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CAN HORMONES HELP WIN THE PEACE?

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"WINNING the peace" is a big order, political and social in nature, and the possibility that hormones may have a place in it will seem an exceedingly remote one to many. Particularly will it appear so to the layman, who regards the endocrine glands as a bit of the medical profession's academic property.

But one of the ramifications of winning the peace is the activity of rehabilitation. The purpose of this activity is the rebuilding of cities, and more important, the restoration of the health of the people who are to inhabit them. These people will have lived for a long time under conditions of terrible privation. Few, if any of them, will have escaped periodic or sustained hunger, and most of them, particularly the children, will be suffering from malnutrition varying in degree and kind. If at this point the question were asked whether vitamins could help win the peace, even poorly informed people would answer affirmatively. All who can read the advertisements know of the existence of vitamins, and of their importance in nutritional well-being.

I

From a theoretical point of view, most of us have a rather one-sided idea as to what constitutes nutrition. By a long process of education we have come to think of it in terms mainly of the substances which we must obtain and place at the disposal of our digestive tracts; that is, the essential foodstuffs. In a practical sense this emphasis upon the *extrinsic* phase of nutrition is justified. An individual can apply his knowledge of it directly, in ordering from a menu or in buying food for his family. But when the food has been received by the digestive tract, the processes of nutrition have only begun. The breakdown substances, which are our real food, must be shifted about in the body, altered, and reshifted until they reach the cells which are finally to use them. In his outstanding book on malnutrition, published in 1925, C. M. Jackson referred to these complex internal dealings as *intrinsic nutrition.*[1] And the basic influences controlling this part of nutrition, according to the same author, are heredity, toxins, *and the hormones.*

In the eighteen years since the two phases of nutrition were thus defined, an extensive new content has been put into the concept, especially in so far as the hormones are involved. This is because the larger part of our knowledge of the endocrines has been established in these years, *and to a surprising extent this knowledge is concerned with what happens to food substances once they have entered our bloodstreams.*

Let us consider, by way of example, what may happen to a single foodstuff. Some carbohydrates, when completely digested, give rise to the simple sugar

[1] C. M. Jackson, *Inanition and Malnutrition.* P. Blakiston & Co., Philadelphia.

glucose, which is the ideal fuel of our cells. After the glucose has entered the blood, it is picked up by the liver cells and stored away, to be released into the blood gradually during the intermeal period. If one has a diseased pancreas and therefore is deficient in *insulin,* this storage does not occur; much of the glucose is washed away in the urine, and the body cells have to use other substances as fuel. New shortages are thus created, and the diabetic victim wastes away. The cells do not stop burning fuel on account of the glucose shortage; they merely switch to other fuels and tend to continue metabolizing at the previous rate. This rate, in turn, is governed to some extent by several endocrine glands, especially the thyroid. If one's thyroid is too active the sugar is burned too fast, and again there is a supply problem. Again the cells are forced to use proteins for fuel, and emaciation may result. On the other hand, if one's thyroid is underactive, the sugar burns too slowly; there is a glut of sugar which is usually converted into fat. This is the cause of one type of obesity. Another gland which is concerned with the use of glucose is the adrenal. If the adrenal cortex is underfunctioning there will be too little glucose in the blood. If the adrenal medulla is defective, the blood cannot receive the little bursts of glucose from the liver which presumably help one to withstand sudden stress. Finally, if one's pituitary is functioning badly, the sugar is mishandled in other ways, depending on the nature of the disorder. *Thus, the distribution and use of glucose within the body are controlled by at least five endocrine glands.* Other food substances are probably subject to similar influences.

Similary, gross structures of the body, such as bone, are affected in growth and differentiation by individual and synergistic action of hormones. This being so, it is clear why disorders of the anterior pituitary can bring about such drastic changes in an individual. This organ secretes a miscellany of hormones: one which keeps the adrenal cortex in normal size and function (*corticotropin*); one which stimulates the thyroid (*thyrotropin*); several which maintain the sex glands (*gonadotropins*); and some others, which, like the growth hormone *somatotropin,* produce general effects. Slight periodic shifts in the relative output of these hormones, in normal animals, bring about sequential events such as menstruation, mating seasons, and migrations. Pathological changes in the pituitary cause profounder shifts, which may produce dwarfs or giants, human skeletons or human gorillas. All such phenomena represent changes in intrinsic nutrition, of which bodily size, shape, and (in the narrow sense) behavior are only the outward expression.

Upon the basis of such indications it might be thought feasible to try hormones empirically, as drugs often are tried, to improve the nutritive processes of starvation victims. But the hormones are powerful drugs, and their use may involve dangers, especially in organisms whose metabolic activities have become abnormal. It would be better to try first to answer the following questions: *first, whether chronic malnutrition has lasting effects upon organisms; second, whether these effects may be due in part to endocrine injury; and third, whether hormone therapy gives any promise of repairing the injury or of sustaining the organism until spontaneous recovery can occur.*

II

For five months of the 1914–1918 War, fourteen thousand British Empire soldiers were besieged by the Turks at Kut, Mesopotamia. During the siege their rations were only one-third to one-half of the normal quantity. Death from starvation was common, and every man

was severely emaciated when the siege ended. The medical end of the grim story was related several years afterward by Major-General Sir Patrick Hehir. After noting that the return to normal diet must be very gradual (a full meal sometimes being fatal) he wrote:

> It was a long time before those who went through the siege . . . regained normal weight; in my own case it was one and a half years. This appears to be the record of all long sieges. . . . The digestive glands of the stomach, the pancreas, and liver, have undergone considerable atrophy, and are long in recovering their normal functioning activity. It was about two years before I personally could consume a normal meal as regards quantity. . . .[2]

This passage is quoted to emphasize the deep-seated effects of starvation, and the long time required for recovery. If strong men, starved for five months, take two years to recover, how long will it take the children of China and Europe who have starved for three to five years?

As a matter of fact, very little is known about the effects of chronic malnutrition in human beings, particularly during and after the resumption of feeding. Genetic differences in size and constitution are so great that an honest physician would hesitate, several years after famine, to ascribe the short stature of one individual or the illness of another, to malnutrition in the past. In summarizing the available clinical and experimental evidence, Jackson comments, "Between these upper and lower limits of inanition [weakness from starvation], there is probably in all cases a degree of injury possible which permits only of partial recovery, resulting in a variable degree of dwarfing . . . of the body." This has been borne out abundantly by experiments with animals. In rats, which have strong powers of recuperation from malnutrition, a diet deficient only in calories may result in permanent dwarfing. Such animals have been described as having humped backs and bulging eyes; such abnormalities of proportion are corrected by normal diet but full size will not be attained. It has been observed that when five-week-old rats are deprived of minerals or calories, an irreversible stunting sets in three to six weeks later—a time roughly equivalent to human childhood. If refeeding is instituted before this effect sets in, the resumed growth is abnormally rapid. Such overcompensated growth might easily be due to unbalanced pituitary action.

One feature frequently observed in undernourished young animals is an irregular, or as Jackson calls it, "dystrophic" growth:

> As already stated, the age at the time of inanition is an important factor, there being critical periods at which various organs are most susceptible. . . . During the developmental period . . . inanition (especially of the chronic type) frequently results not merely in a retardation or cessation of growth, but in an abnormal, disproportional growth. . . . Some parts may show persistent growth at the expense of others, even during total inanition with continued loss of weight.

For example, underfed young rats have abnormally long crania. Puppies and young steers which have been kept from growing by partial starvation become too tall and long for their body weight. One author comments, "little can be done to prevent the persistent growth of bones. . . . In spite of stationary body weight, the physique of the experimental animal slowly changes, the length and height of the body increasing and the thoracic cavity becoming deeper and narrower."[3] Whether these effects, like stunting, are permanent, and whether they appear in human beings, we simply do not know. But we are told, with reference to the Jews of Poland a generation ago: "Their physical strength, their muscular power has diminished in each generation; their

[2] Sir Patrick Hehir, "Effects of Chronic Starvation during the Siege of Kut," *British Medical Journal.* 1922, 1: 865–868.

[3] Arthur H. Smith, "Phenomena of Retarded Growth," *Journal of Nutrition.* 1931, 4: 427–441.

blood is poor; their stature is small, shoulders and chest narrow."[4] Perhaps there is a physiognomy which hunger stamps upon its victims; if so, it is caused by disproportionate growth, a subject which always interests the endocrinologist.

As for the question whether malnutrition affects the endocrine system, the evidence seems fairly definite, although it is not known how long the effects remain. Again going back to the last war, we find this testimony from a young British doctor who spent a long time in an enemy prison camp:

> Many batches of prisoners had been systematically underfed, and had been worked until they were physically prostrated. From each batch some members died a very short time after admission. I was greatly impressed, in carrying out autopsies on these emaciated cases, by the size of the adrenal glands. In a series of eight cases of death from underfeeding the adrenals were enlarged, almost half as large again as normal, and the enlargement seemed, from naked eye appearance, to be mostly in the cortex.[5]

The enlargement of the adrenal cortex in crises is now a well-known phenomenon, due either to adrenal damage or to its stimulation by increased *corticotropin* from the pituitary. H. Selye[6] regards it as part of a "general adaptation syndrome" in which the pituitary is upset by threatening environmental conditions in such a way that more corticotropin is secreted, while less of the gonadotropins, which stimulate the sex glands, are put out. He cites the fact that many German women who lacked food during the last war were incapable of menstruation, a sexual function under indirect pituitary control. Adrenal cortical enlargement in acute starvation has tended to obscure the fact that in chronic—moderate and prolonged—malnutrition, the adrenal cortex becomes atrophied. This is true also of the sex glands, the thyroid, and the pituitary itself, as has been shown by autopsies of men as well as of laboratory animals. *Atrophy, or wasting away, of the endocrine glands is a common feature of starvation.*

This has been proved by physiological experiments, especially with regard to the anterior pituitary. Earlier in this paper "human skeletons" were mentioned. In such persons the pituitary gland for unknown causes has withered away, bringing the condition known as "Simmonds' disease." There is an almost indistinguishable disease (*anorexia nervosa*) which seems to be caused by the victim's inability or unwillingness to eat properly. Autopsy in several cases has shown the pituitary to be atrophied. Laboratory animals with characteristics similar to those of "human skeletons" can be produced by removing their pituitaries. This operation is called *hypophysectomy.* In such animals we know that the emaciation, the weakness, the infantile characteristics, and the atrophy of adrenal, thyroid and sex glands, are all due to the loss of the pituitary gland. But in starved human beings we do not know to what extent similar symptoms are due to direct tissue starvation and to what extent to the curtailment of pituitary activity. At any rate, the effects of pituitary removal and those of chronic starvation are so much alike in experimental animals that Mulinos and Pomerantz[7] have spoken of a "*pseudo-hypophysectomy syndrome.*" This scientific mouthful means, "the

[4] Sergius Morgulis, "Fast and Famine," *Scientific Monthly.* 1923, xvi: 54–65.

[5] Charles H. C. Byrne, "Enlargement of the Adrenal in Starvation," *British Medical Journal.* 1919, 2: 135.

[6] Hans Selye, "Effect of Adaptation to Various Damaging Agents on the Female Organs of the Rat," *Endocrinology.* 1939, 25: 615–624.

[7] Michael G. Mulinos, and Leo Pomerantz, "Pseudo-hypophysectomy," *Journal of Nutrition.* 1940, 19: 493–504. *Ibid.*, "The Reproductive Organs in Malnutrition," *Endocrinology.* 1941, 29: 267–275. *Ibid.*, "Hormonal Influences on the Weight of the Adrenal in Inanition," *American Journal of Physiology.* 1941, 132: 368–374.

combination of symptoms (in starved animals) which simulates the set of effects occurring in animals whose pituitaries have been removed."

The symptoms referred to include such things as the loss of the estrus cycle in female mammals. This loss indicates a deficiency in the animals' sex glands, and the latter in fact are found to be atrophied in underfed rats, guinea pigs, and chicks. In some of the experimental animals, injections of the proper sex hormones cause a regrowth of the sex organs, while gonadotropins bring repair to both sex glands and organs. Fertility is sometimes restored. *It is now fairly certain that the wasting away of some of the endocrine glands during malnutrition is due less to the direct starvation of their tissues than to the failure of the pituitary to support them with its "tropic" hormones.* Hence the results of pituitary removal and starvation are similar because the latter, in effect, removes part of the pituitary. That the same may be true in some human cases is shown in the conclusion of one medical authority, that "many of the clinical manifestations of *anorexia nervosa* and other types of chronic inanition are due to the effects of undernutrition of the anterior hypophysis."[8]

The third question, whether hormone therapy gives any promise of repairing the damage from malnutrition, has been partially answered by implication in the foregoing paragraph. Even though the male rat continues to be underfed, its genitalia are repaired by gonadotropin or testis hormone. When extra pituitary glands are transplanted into underfed female rats, the estrus cycle and the adrenal cortex are restored, despite continued underfeeding. This, however, is the highwater mark of experimentation along these lines. No one has investigated the resumption of growth in re-fed animals during hormone therapy, in comparison with the growth of animals merely re-fed. No one has tried hormone therapy upon animals "permanently dwarfed" by malnutrition. With regard to the problem of human malnutrition there is little point to hormone therapy experiments *without simultaneous re-feeding.* We need to know, particularly, the effects of hormones with growth, differentiating, protein-saving and bone-metabolizing properties upon animals which have first been starved and then put upon a full and adequate ration.

Even here, however, there are some oblique indications of the possibilities. Several kinds of dwarfism have been treated successfully with pituitary and sex hormones. Physically retarded children have been induced to grow better, through the administration of gonadotropic and sex hormones. An interesting case[9] may be cited. An infantile, emaciated eighteen-year-old boy, who had not grown for eight years, was treated with anterior pituitary extract. In four and a half months he became sexually mature and grew two inches taller (during this time his normal sixteen-year-old brother, serving as a sort of control, had not grown measurably). After treatment he continued to grow and gain weight. It may be postulated that in these successfully treated victims of arrested growth, intrinsic nutrition had been deranged by unknown, perhaps genetic, causes. Since hormone therapy was effective, it may be postulated further that some of the intermediate causes were endocrine in nature. And if hormones can work in such stubborn material, we should not overlook the possibility that they might also be effective in organisms whose intrinsic nutrition has been altered (*via* endocrine impairment) by poor diet.

[8] D. J. Stephens, "Anorexia Nervosa. Endocrine Factors in Undernutrition," *Journal of Clinical Endocrinology.* 1941, 1: 257.

[9] Max M. Goldberg, "The Treatment of Pituitary Infantilism with Anterior Pituitary Extract," *Endocrinology.* 1936, 20: 854–855.

III

To refer back to our three questions, it should be noted that they have been answered affirmatively, not in a scientifically conclusive way, but with bits of suggestive evidence. But the evidence holds together and forms a chain of which the chief links are facts well accepted in the fields drawn upon. It is believed, furthermore, to indicate the possible existence of a new opportunity to serve a portion of humanity which most needs help. What, then, can be done to uncover and develop this opportunity?

First of all, we can learn a great deal from animal experimentation. The basic procedures and techniques have been developed. The planning of the experiments involves no unusual difficulties, and their accomplishment requires only the usual careful routine work familiar to nutritionists and endocrinologists. These experiments would tell us whether it would be safe to go ahead, or whether the idea of hormone therapy in malnutrition is, as well it may be, a dangerous mirage.

As to the problem of treating human beings, great difficulties may arise. Whether or not cautious empirical trials would be justified is a question which can be answered only by competent medical authorities. There may be regional differences in the types of malnutrition incurred, and hence in the resulting endocrine damage. In a given region, individuals may suffer differently. Effort would have to be made to determine the nature of such regional and individual differences, and treatment would have to vary accordingly.

If this sounds like a formidable program, it must be emphasized that malnutrition is not a transient problem. In its severe form, it may be of concern to us for a decade. We may, unfortunately, have more time than we would otherwise desire to have, to solve the questions raised.

A generation ago, at the time of another war, our knowledge of vitamins was even more rudimentary than is our present knowledge of endocrine physiology. To harassed post-war Russia the American Relief Administration sent, among other supplies, only one vitamin preparation.[10] During and after the present war we shall send the world's starving peoples many kinds of food and vitamins, carefully evaluated for their potency in restoring human beings to good health. Whether or not hormones will also be sent, no one can say. The study of their effects in starved organisms will some day be undertaken in the course of normal scientific activity. But why should we not accelerate this program, as we have so many others?

[10] Harold Henry Fisher, *The Famine in Soviet Russia, 1919–1923; The Operations of the American Relief Adminstration.* Stanford University Press, 1935.

VISUAL ORGANS OF INVERTEBRATE ANIMALS

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FEW organs in the animal body have undergone so remarkable an evolutionary development as the visual organs, which have developed into several types in different groups of animals. Although the image-forming eyes of vertebrate animals are all fundamentally alike, the visual organs of invertebrate animals form seven well-defined types: light-sensitive spots in unicellular animals, light-sensitive body surfaces, light-sensitive cells in the body wall, pigment-cup ocelli, arthropod ocelli, simple eyes, and compound eyes.

These different types of visual organs, however, do not represent an orderly evolutionary series. Animals with light-sensitive cells in their body walls occur in all the major phyla of animals above the Protozoa, which suggests that the members of different phyla may have evolved their own particular type or types of visual organ from visual cells of the body wall. Among invertebrate animals there is often little correlation between the degree of specialization of the visual organ and systematic position of the animal that bears it. In certain phyla, as in the Mollusca, there are found many different types of visual organs, ranging from simple visual cells in the body wall to image-forming eyes.

For the most part, the visual organs of invertebrate animals are concerned chiefly with the perception of light and hence are often called *photoreceptors.* The term *eye* is usually applied only to those visual organs having a light-sensitive layer of cells, the retina, on which light rays from external objects are focused. One must not judge the visual powers of an animal by the structure of its visual organ alone, but must also consider the brain mechanism that is connected with it.

The lower invertebrate animals are often limited in their visual functions to the perception of light, but the higher invertebrate animals can often perform as many as six different functions from information which they receive from their visual organs. They can detect light (distinguish light from darkness), distinguish between different intensities of light, determine direction of light, determine depth and distance, determine form (image-forming eyes), and distinguish colors (color vision). In general these different functions represent stages in the functional evolution of visual organs, although the mechanisms by which they operate may be quite different in different animals.

If the *presence of light* cannot be detected, it is obvious that none of the other above-mentioned functions can exist. If light can be detected, *intensity discrimination* may also occur without additional morphological differences in the visual organs. This is shown by *Amoeba* and by certain other animals in which the surface of the body is sensitive to light.

In order for an animal to be able to determine the *direction of light* it is necessary for certain regions of the body to be more sensitive to light than other regions, or for certain visual cells to be stimulated to the exclusion of others, as in the earthworm, leech, *Planaria* (Figs. 1A, 1B, 1C) and certain other animals. In some animals pigment partly surrounds the visual cells so that light can enter from only one direction, and in

others lenses serve to focus light upon the visual cells, as in the ocelli of insects. Such visual organs enable their possessors to determine the location and movement of objects.

The perception of *distance* usually demands binocular vision, as in man, but the visual organs of certain arthropods and mollusks seem well adapted for this purpose. Since the eye of the scallop, *Pecten* (Fig. 4A), has two separate retinas (proximal and distal), it is probable that light rays from a distance and those from nearby are focused on different retinal layers. The situation in certain myriapods (Fig. 2A) is quite similar, since their visual organs have several layers of retinal cells. Human beings can judge distance with one eye, only if they know the size of the object and consequently have some judgment of the angles it would subtend at different distances. While binocular vision is usually a prerequisite for judging the distance of objects, it is probable that a few animals without this type of vision can nevertheless judge distances.

The perception of form (image-formation) is possible only if the aperture through which light enters the eye is very small, as in *Nautilus* (Fig. 3B), or if a lens is present and forms an image on the light-sensitive retina. To form clear images of objects at different distances it is necessary for the eye to have the power of accommodation. The clearness of the image also depends on the structure and shape of the lens.

There is evidence that some of the higher invertebrate animals, such as bees and certain other insects, as well as many vertebrate animals, possess the power of color vision. As is well known, visible light is composed of waves of various lengths. Long waves affect certain retinal cells, resulting in the sensation of red; shorter waves stimulate other retinal cells, resulting in the sensation of green, and still shorter waves, blue.

Light-Sensitive Body Surfaces

In the simplest animals a specialized light-sensitive structure is not necessary, since the protoplasm of the entire body surface is sensitive to light. When *Amoeba* is exposed to strong light no pseudopodia form on the exposed side. *Amoeba* is equally sensitive to light over its entire body, but many of the more specialized Protozoa are more sensitive to light in certain regions than in others. In such organisms as *Euglena,* for instance, photosensitivity is limited to the eye-spots.

Planaria from which the visual organs have been removed react to light in much the same way as normal animals. Similarly many vertebrate animals, including certain fishes and frogs, which possess visual organs, have been shown to have also a "skin" sensitivity to light. Even though these animals are blinded, they still possess the ability to respond to light. It has been supposed that free nerve endings in the skin serve as receptors. However, recent work on *Dolichoglossus* and *Crangon,* in which light-sensitive cells were found in the outer body wall, suggests that light-sensitive cells rather than free nerve endings may act as receptors in most, if not all, animals having a "skin" sensitivity to light.

Light-Sensitive Cells in Body Wall

The work of numerous investigators shows that most, if not all, metazoan animals having light-sensitive body surfaces have cells in the body wall that are stimulated by light. Such names as photoreceptors, visual cells, retinulae and rod cells have been applied to these sensory structures. For purposes of uniformity we shall refer to them as visual cells.

Visual cells which are more or less unspecialized in structure but which function as photoreceptors are found in the outer body wall of many coelen-

terates, echinoderms, annelids, mollusks and arthropods, as well as in certain vertebrates. Although many of the lower coelenterates, such as *Hydra* and sea anemones, respond to light, visual cells have never been found in their body walls. In certain jellyfishes and many other animals, pigment cells alternate with visual cells. Simple visual organs may be found in the higher, as well as in the lower, animals and simple as well as complex organs may be found in the same animal.

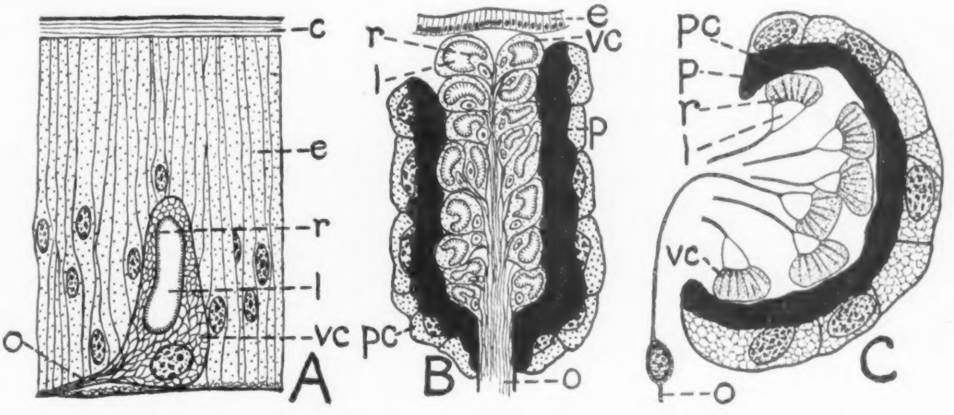

FIG. 1. VISUAL CELL OF BODY WALL AND PIGMENT–CUP OCELLI
(A) VISUAL CELL OF EARTHWORM BODY WALL; (B) PIGMENT-CUP OCELLUS OF LEECH; (C) PIGMENT-CUP OCELLUS OF *Planaria;* (c) CUTICLE OF THE COMMON EARTHWORM, (e) EPITHELIUM, (l) LENS, (o) OPTIC NERVE, (p) PIGMENT, (pc) PIGMENT CELL, (r) RETINELLA, (vc) VISUAL CELL.[1]

Since the visual cells of the common earthworm, *Lumbricus terrestris* (Fig. 1A), are better known than those of most other animals possessing visual cells in the body wall, we will use them as examples of this type of receptor. They are found near the base of the outer body epithelium of all body segments and in nerve enlargements of the prostomium and the caudal segment. However, the largest number is found in those regions that are most photosensitive, especially the prostomium. Each visual cell contains an inner cylindrical lens, which is surrounded by a neurofibrillar network, the retinella. This lens is usually bent so that different portions of it lie in different planes. Consequently, it usually focuses light on portions of the retinella irrespective of the direction from which the light comes. Therefore the neurofibrillae of the retinella appear to be the direct receptors of light stimuli. The long-necked clam, *Mya arenaria,* has similar visual cells located in the walls of its siphons.

Many of the polychaete worms, such as *Nereis virens,* also have, in addition to simple eyes, visual cells in the outer body wall which resemble those of the earthworm in structure. In the primitive chordate, *Dolichoglossus kowalevskyi,* visual cells resembling rods and cones in shape are found at the base of the epithelium of the body wall.

Visual cells, such as have just been described, function in the detection of extremely small amounts of light, as well as in detecting differences in intensities. Because of the location of their visual cells, these animals are also able, to some extent, to detect the direction of light. This is shown by the fact that when

[1] Some of the figures used in this paper were redrawn with modifications from the works of earlier authors.

earthworms are experimentally illuminated on one side only they turn away from the light. It was not, however, until pigment-cup ocelli evolved that animals could determine the direction of light with any degree of accuracy, and it was not until simple eyes evolved that is that the lens and retinella are differently located within the cell.

The size of the openings of pigment-cup ocelli differs considerably in different animals. It appears possible that a perception of light direction is obtained in those animals of the pigment-cup

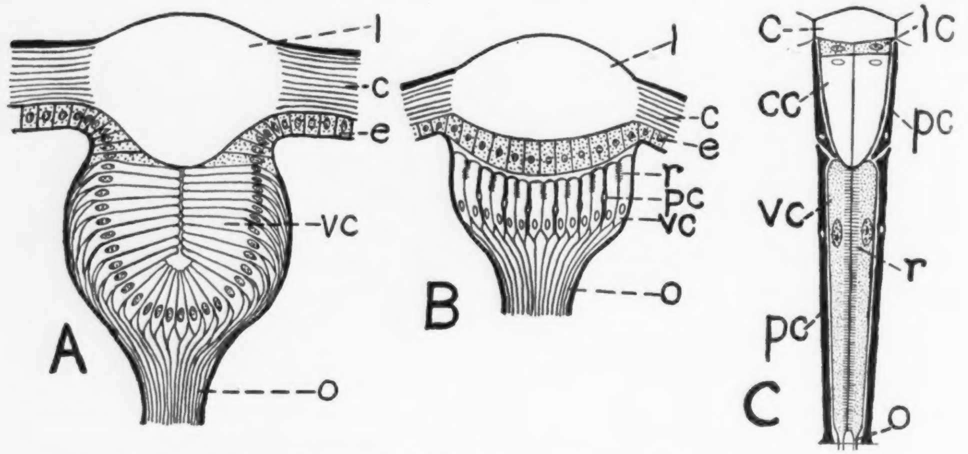

FIG. 2. ARTHROPOD OCELLI AND OMMATIDIUM OF COMPOUND EYE
(A) OCELLUS OF MYRIAPOD (*Heterostoma*); (B) OCELLUS OF INSECT; (C) OMMATIDIUM OF COMPOUND EYE OF INSECT; (c) CUTICLE, (cc) CRYSTALLINE CONE, (lc) LENS-SECRETING CELL, (e) EPITHELIUM, (l) LENS, (o) OPTIC NERVE, (pc) PIGMENT CELL, (r) RHABDOM, (vc) VISUAL CELL.

animals were able to determine the form of objects.

Pigment-Cup Ocelli

It was in the flatworms that Nature first massed together near the anterior end of the body visual cells of the type that we have just described for the earthworm and partially surrounded them with a cup of cells containing pigment. Thus was formed our third type of visual organ, the pigment-cup ocellus, which is found in many leeches and flatworms and in some of the early chordates, such as *Amphioxus*. While the visual cells within the pigment-cup ocelli of the flatworm, *Planaria maculata*, appear to be superficially different from those of the earthworm and leech, they possess the same intracellular lens and retinella (Fig. 1C). The only essential difference

type when the opening to the exterior is very small. However, it has been shown that the individual visual cells of the pigment-cup of *Planaria* are stimulated only when their axes are in the direction of the incoming light. This indicates that it is not the pigment-cup but more especially certain structures within the visual cells which enable this animal to detect the direction from which light enters the ocellus.

The pigment-cup ocellus may be formed of the pigment-cup and only one visual cell, as in *Amphioxus* and certain flatworms, or it may contain many visual cells as in leeches and *Planaria* (Fig. 1B, 1C). In the pigment-cup ocellus of *Planaria*, the sensitive region of each visual cell, the retinella, lies between the lens of the cell and the pigment-cup, the farther end of the visual cell being di-

rected away from the incoming light. These visual cells, therefore, resemble the retinal cells of vertebrates in that they are inverted. Animals possessing visual organs of this type are able to distinguish light and its relative intensity and direction much more accurately than those with the receptors in their body wall. Hence, these visual organs are called *direction eyes.*

There is no evidence that any of the more specialized types of visual organs evolved from pigment-cup ocelli. Indeed, there is much evidence against such a hypothesis.

Arthropod Ocelli

As we pass from animals with pigment-cup ocelli, we come to others having two quite different types of visual organs, both of which are in the general pattern of miniature camera eyes. One of these types of visual organs has a vesicle which usually contains a vitreous body and a lens (Fig. 3C). The visual organs of many echinoderms, annelids and mollusks are of this type and are known as *simple eyes.* The other type of visual organ has no vesicle, and the thickened cuticle acts as a lens. Visual organs of this type are often called the *concentration-lens type* because their lenses usually cause a concentrated spot of light to fall on the visual cells. Visual organs of this type are common among many arthropods, particularly insects and spiders, and hence are known as *arthropod ocelli.* While these ocelli vary considerably in details, their essential features are similar (Figs. 2A, 2B). The visual cells may remain in the epithelial layer or they may lie just below it. Sometimes the visual cells occur in groups with a rhabdom along their inner edges. There is evidence that these ocelli function primarily in the perception of the intensity and direction of light.

Ants with only their ocelli uncovered behave as if blind. The honey bee and the fruit fly (*Drosophila*) both respond more readily to changes in light intensity if their ocelli are uncovered.

The ocelli of the caterpillars of certain moths and butterflies are not true ocelli but are isolated ommatidia of degenerate compound eyes.

Simple Eyes

Since visual organs known as simple eyes are found in echinoderms, annelids and mollusks, they are rather widely distributed among invertebrate animals. These eyes vary in structure from invaginated pits (Fig. 3A) to the specialized visual organs of *Murex* and the squid (Figs. 3C, 4B). They have evolved from patches of visual cells in the outer body wall, forming first invaginated pits and finally simple eyes with a vesicle and lens. These changes can be followed in present-day coelenterates and mollusks.

The visual cells (retinal cells) of most simple eyes are enclosed within a vesicle, with their distal ends directed toward the incoming light rays. There are, however, a few simple eyes among invertebrate animals in which the visual cells are inverted, as in the vertebrate eye. This is the condition in *Pecten* (Fig. 4A) and in the ocelli of certain spiders.

The lenses of these simple eyes are usually formed by the secretion of certain cells and hence are not cellular structures. In the eye of *Pecten* and certain jellyfishes cellular lenses occur.

The simple eye of the chambered nautilus (Fig 3B) has neither a lens nor a vitreous body. Since the vesicle opens to the exterior by a small pit, the eye apparently functions as a pinhole camera. It seems probable that this eye represents a retrogression from a more specialized type of visual organ rather than an early stage in the evolution of a simple eye.

In certain arthropod ocelli and in some eyes of vertebrates the cells back of the visual cells contain a strongly reflective crystalline material which acts as a reflector, the *tapetum*. It is found in the eye of *Pecten* (Fig. 4A) and many other animals, particularly those of nocturnal habits. It is well developed in many moths, ungulates, carnivores and cetaceans. The eyes of cats show the reflection of light from the tapetum by glowing like balls of fire when light from an automobile flashes into them. The exact significance of the tapetum is not well understood. It seems certain, however, that the tapetum reflects light back through the visual cells upon objects that are in front of the eyes and thus may increase their discernibility.

Simple eyes are not limited to invertebrate animals since in certain vertebrates a median simple eye, the pineal eye, is found on the dorsal side of the head. It is a vestigial structure in many adult vertebrate animals but is functional in the primitive lizardlike animal *Sphenodon punctatum*.

In only a very few simple eyes of invertebrate animals, such as the more specialized cephalopods (squid, Fig. 4B), are there an iris and a pupil. Not until we reach the vertebrates do we find true crystalline lenses, eyelids, muscles for moving the eyes in sockets, and an effective mechanism for accommodation.

Simple eyes function in the detection of light, its intensity and direction. Many of them also serve their possessors for determining the distance of objects and as rudimentary image-forming eyes. Since all power of accommodation is usually lacking, it is probable that the perception of clear images rarely occurs.

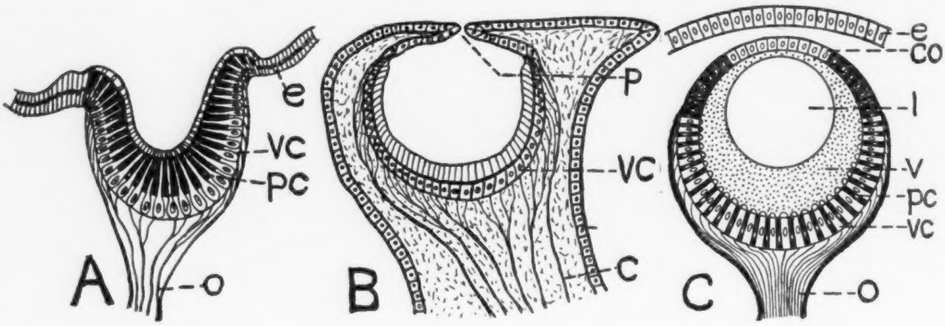

FIG. 3. SIMPLE EYES

(A) PITLIKE VISUAL ORGAN OF LIMPET (*Patella*); (B) PINHOLE TYPE OF SIMPLE EYE OF CHAMBERED NAUTILUS; (C) SIMPLE EYE OF SNAIL (*Murex*); (co) CORNEA, (e) EPITHELIUM, (l) LENS, (o) OPTIC NERVE, (p) PINHOLE, (pc) PIGMENT CELL, (v) VITREOUS BODY, (vc) VISUAL CELL.

Compound Eyes

Many arthropods, chiefly insects, possess two types of visual organs, ocelli and compound eyes. Sometimes both types are found in the same animal. Many of these ocelli differ from compound eyes chiefly in the fact that the single ocellus possesses one focusing apparatus (lens) for all its visual cells, while compound eyes, which are composed of many units (ommatidia), have a separate focusing apparatus for each unit.

As shown in Fig. 2C each ommatidium is composed of retinal or visual cells, pigment cells, and a fixed focusing apparatus composed of a separate lens (crystalline cone) and a cornea. The edge of each retinal cell has a striated zone composed of the ends of many neurofibrillae, which are connected with

the optic nerve. The striated borders of all the retinal cells of an ommatidium which lie in apposition with each other are called a *rhabdom*. Although the compound eye has no power of accommodation, such as occurs in the eyes of most vertebrate animals, it is possible that the elongated rhabdom is an effective substitute since rays of light from both near and distant objects are focused upon it.

A single ommatidium of a compound eye and the individual retinal cells of vertebrates each receive light from a very small area of the outside world, and consequently the ommatidia are in that way comparable to the individual retinal elements of the vertebrate eye. Animals with compound eyes possess what is called *mosaic vision*. All vision, however, is probably mosaic, since perception of an entire object is dependent on the sum of the individual stimulations of numerous ommatidia or retinal cells.

Possessors of compound eyes react more to motion than to the details of images as in man, although insects have been observed trying to take nectar from flowers on wallpaper.

Among present-day species well-developed compound eyes are found only in crustaceans and insects, yet in a few sea-urchins (*Diadema*) a simplified compound eye occurs.

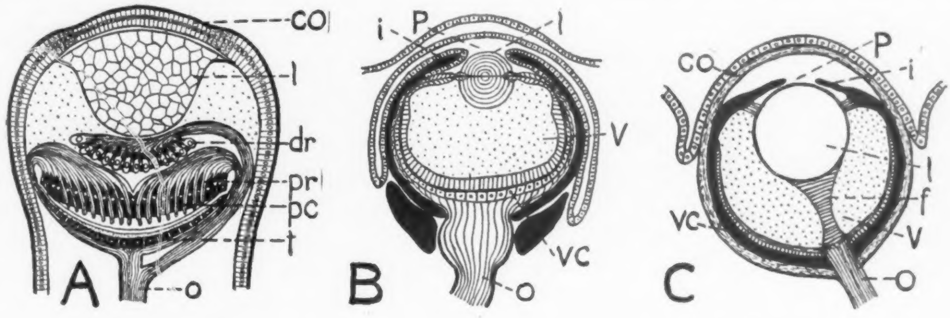

FIG. 4. SPECIALIZED TYPES OF SIMPLE EYES AND VERTEBRATE EYE
(A) EYE OF SCALLOP, DOUBLE RETINA; (B) IMAGE-FORMING EYE OF SQUID; (C) EYE OF VERTEBRATE; (co) CORNEA, (dr) DISTAL RETINA, (f) FALCIFORM PROCESS, (i) IRIS, (l) LENS, (o) OPTIC NERVE, (p) PUPIL, (pc) PIGMENT CELL, (pr) PROXIMAL RETINA, (v) VITREOUS BODY, (vc) VISUAL CELL.

Accommodation Eyes

The power of accommodation, or the adjustment of the eye for perceiving distant and near objects, is generally considered to be a possession of only vertebrate animals. There is some evidence, however, that in the eyes of certain of the higher mollusks, such as the squid, the contraction of the sphincter muscle of the iris increases the pressure on the vitreous humor, which in turn causes the lens to be pushed forward, thus changing the focus of the eye from far to near.

Two methods of accommodation evolved in fishes. In many of the elasmobranchs the lens is pulled forward by muscles of the iris for the perception of near objects. When these muscles relax the lens automatically moves back to adjust the eye for far vision. In the second method the lens is pulled backward, for viewing distant objects, by a muscular band which extends from the region of the optic nerve to the surface of the lens, the falciform process (Fig. 4C). When this relaxes, the eye automatically readjusts for viewing near objects. The falciform process is especially well developed in many bony fishes, such as the genus *Salmo*. In amphibians, reptiles and birds the falciform process, now called the pecten, is no longer attached to the lens but is still connected to the optic

nerve. In man the falciform process is rarely present in adult life but is present during fetal life as a thin, gray, vascular cord, which usually degenerates shortly after birth. However, it occasionally remains throughout life as a vascular cord extending from the optic nerve to the lens in the path of the old faciform process of fishes.

When the eye of man is at rest, the elastic choroid coat of the eye exerts a pull on the suspensory ligaments attached to the lens, causing it to become flattened. To accommodate the eyes for near objects contraction of the ciliary muscles counteracts the pull of the choroid coat, and consequently the lens through its own elasticity becomes more convex. In this way the eye is accommodated for viewing objects at different distances by changing the curvature of the lens.

Thus we see that visual organs with certain powers of accommodation probably did not make their first appearance in the vertebrates but apparently arose in certain mollusks closely related to the squid. However, accommodation by changing the convexity of the crystalline lens occurs only in vertebrate animals.

Vertebrate Eye

As we pass from simple eyes to true accommodation eyes a great change in the eye at once becomes apparent. Where did this greatly improved eye originate? Some think that it was from arthropod ocelli, others from simple eyes, and still others from the ocelli of arachnids having inverted retinal cells. Nevertheless, it seems quite probable that vertebrate animals did not inherit their visual organs from the higher invertebrates, but that they developed eyes of a new and better type which involved three major differences: (1) a change in the origin of the visual cells (retina) from the outer body epithelium to the brain, resulting in an inverted retina; (2) a great increase in the number of retinal cells, with a differentiation of them into rods and cones; and (3) the development of the accommodation lens.

Representatives of all the major groups of animals, except Protozoa, are known to have visual cells in the body wall. Now that the primitive chordate, *Dolichoglossus kowalevskyi*, has been shown to possess visual cells in the nerve plexus of its outer body wall, it seems even more probable that the organs of vision in the vertebrates evolved from the visual cells in the body wall of the early chordates. It is probable that with the development of the central nervous system from the outer body epithelium of the early vertebrates the visual cells became incorporated in the central nervous system, as in *Amphioxus*, and later became localized in the region of the brain which forms the retina of the vertebrate eye.

WHY PUBLIC HEALTH EDUCATION?

By RUTH ALIDA THOMAS

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THE holocaust of war is tearing away the last remnants of a smug national complacency and is opening our eyes to defects in our social structure. In the field of public health, the shortcomings of the present stand out in bold relief against the possibilities of the future. We are apprehensive about the shortage of civilian doctors and nurses, but loath to admit that in the past we too often relied upon them to patch up the consequences of our own disregard of basic health principles. We are fearful that an epidemic will flare up in one of our overcrowded defense areas before we can muster our depleted resources for its control. We are aghast at the number of rejections reported by Army and Navy examiners for minor defects, the majority of which could have been prevented if adequately cared for in childhood. As the result of the present emergency we have overcome our lethargy, but the very speed with which we have had to plan for the health and welfare of both service and civilian groups necessitates a critical evaluation of objectives.

Leaders in the fields of medicine, of sanitation and of public health have worked miracles in the past forty years in reducing the mortality from many of our common communicable diseases. The typhoid death-rate has dropped from 35.9 to 1.5, diphtheria from 43.3 to 1.5 and tuberculosis from 203.1 to 48.7, and yet all are still unnecessarily high. These servants of the public have taught us the importance of a balanced diet and have materially raised our standards of nutrition; they have provided our babies with a better and safer start in life. But in spite of these accomplishments, we have scarcely begun to sound the possibilities. As a result of the war, health has become, as never before, a community problem. We are learning that the construction of barriers against the spread of epidemics is not enough—that a nation in need of manpower must be concerned with the positive health of each and every citizen. Experience gained in meeting the needs of the present has made us realize that the projection of a vital, constructive post-war program will require the active participation of an awakened citizenry. Looking to the long term, it is evident that the most productive means of acquiring the cooperation of Mr. and Mrs. Citizen is in effective teaching in the schools and, particularly, in the colleges. Although rapid strides have been made in health education in recent years, yet in many parts of the country the actual health knowledge of the average adult remains woefully inadequate. It is time to take stock of our present resources, and having set our goals, to direct our education to that end.

The United States Office of Education is meeting the immediate problem by including education for health as a vital part of the Victory Corps program. Under the slogan, "health for victory," this program has wisely been developed for the high schools, since they are the acknowledged centers for the training of boys and girls, not merely for war service but for life. The objectives, as outlined, are comprehensive, even though they are drawn up with the primary purpose of making our high school youth physically fit for participation in various wartime activities. The ultimate success of this program will depend upon its execution—upon the quality and scope of the teaching involved. With motivation so strong, we must guard against the danger that it will be super-

ficial or one-sided. Given a sound scientific background and an understanding of the potentialities for or against health in ordinary life situations, such a program should have a "carry-over" into peace time which will materially increase the health of the average citizen.

We are building adequately for the future, however, only if we also instill a sense of social consciousness and community responsibility. One of the characteristics of our modern civilization is that we are increasingly interdependent upon one another. The bright future which we envision for the post-war world will depend to a very considerable extent upon our concepts of public health—on constructive health measures beyond the control of the individual. We should no longer speak apologetically of our rural areas as "weak spots" but, recognizing that directly or indirectly we are each affected by health conditions beyond our immediate environs, we should be prepared to spend State or Federal funds to assure our country cousins' health benefits comparable to those provided in our most progressive cities. With a new interest in the health of our future citizens, we should not question the need for extension of our maternal and child health programs. Recognizing that many of our delinquency problems and a large proportion of the cases now cared for in mental institutions at public expense can be referred back to maladjustments capable of correction if studied in time, we should make use of the abundant knowledge being acquired through psychiatric research under Army and Navy auspices in building up community mental hygiene programs. We should face the problems connected with the non-communicable diseases peculiar to older age groups, to ascertain in what respects they are community problems.

The "post-war" world can be the "golden age" of public health if we give thought now to the appropriate education for the prospective citizens whose support we will so urgently need. In the inevitable shift from destruction to construction, the time will be ripe for an acceleration of our present efforts in the public health field. We will have an opportunity then, not only to strengthen our present work, unfortunately often demoralized by shortage of trained personnel, but to intensify our endeavors. We should be prepared to accept constructive health work as one of the functions of democratic government—as distinct from state medicine on the one hand and philanthropy on the other. Men returning from the Army and Navy, particularly the medical officers, will undoubtedly have become impressed with the values to be gained in health through community planning. Moreover, the great effort needed to maintain our civilian health services through the difficult war years, should be productive of far-reaching benefits. It will be worthwhile if it precipitates a reshifting of emphasis in the work within each community in accordance with the seriousness of the problems and the chances for solution, and if it effects a closer tie-up between professional and voluntary agencies.

If we believe that there is room for accentuation and extension of our present public health programs in the post-war world, it is time for us to begin to plan for it. Through education we can train our young people to be alert to the health problems of their communities—to think in terms of social consciousness and communal responsibility.

The Victory Corps program should stimulate new interest in health education at the secondary school level, but on our colleges and universities will fall the responsibility of developing leadership. Already we find ourselves sorely in need of teachers equipped to initiate and carry through vital, stimulating health projects in the schools. We will need more professional workers and legislators with a background of public health to bolster health education in the com-

munities, but even more crucial will be our need for keenly interested citizens. The pace of progress will be proportionate to the public interest. For any effective work we must have key people in the various social groups to help search out and study the problems. We will have to find among the membership of our women's clubs and businessmen's organizations, men and women who, realizing that the local health board should be more than a politically dominated policing agency, will instigate the appointment of alert, progressive officials and then rally their fellow citizens to the support of their projected programs.

The scope of public health must be broadened if it is going to contribute materially to the realization of the opportunities and the way of life for which we are fighting. This will involve more than the protection of the community, by every means known to sanitary and medical science, from the spread of communicable disease. It will pose questions relating to housing, parks and playgrounds, and other esthetic measures whose relation to health cannot be definitely charted without consideration of the economic and sociological factors involved. Another moot question, and one which is already troubling school health administrators and child health workers, relates to the hypothetical borderline between preventive work and correction of defects—the definition of the legitimate boundaries of public health. Standards of nutrition and established measures in environmental sanitation and communicable disease control will have to be reviewed. Solutions will vary according to the character of the community, but each problem will involve not only a careful study by medical and lay experts but the farsighted consideration of each and every citizen, under intelligent and discerning leadership.

The future is bright for progress in the field of public health, but we must not be lax in assuring that future through well calculated education. The leadership of tomorrow will come from among the young men and women now serving in the armed forces, and from the students in our colleges and universities. A brief survey, however, will show that, while many colleges do include courses relating to public health in their curricula, they are frequently considered as vocational and designed primarily for students preparing to embark on a career of community service as prospective nurses and social workers. Without minimizing their value, it can be seen that such courses will have little appeal for the average student who is apt to shy away from them, being adversely conditioned often by repetitious health teaching in the lower grades and by the barrage of health propaganda, frequently misleading and fallacious, which comes to us by way of the radio, popular magazine articles and advertisements. The education which we need must be borrowed from several fields. It should embrace pertinent material from the various sciences and present these scientific facts against the rich background of history, tracing the development of modern medicine, sanitation and public health. It should study them in the light of their sociological import and their implications for social progress. The numerous factors influencing the health problems in different parts of the world should be reviewed and interpreted in terms of their bearing, either directly or indirectly, on our own national health. Such an education would not only help to fit our youth for intelligent, constructive citizenship, but would have outstanding cultural values as well. It is a worthy objective and one which can be attained readily if those interested in public health would endeavor to enlist the aid of school, college and university faculties in thus planning for the future. A nucleus of interest once aroused is bound to expand.

THE GIANT FRESH-WATER FISHES OF SOUTH AMERICA

By Dr. E. W. GUDGER

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In another article I have described, with illustrations from photographs of just-caught specimens, the two largest fresh-water fishes of North America—the huge white sturgeon of the Columbia and Fraser Rivers, and that armor-clad living fossil, the alligator gar of the Mississippi and other Gulf-flowing waters. And now I turn for study to the giant fresh-water fishes of our southern neighbor.

In the great rivers of our twin southern continent are found many fishes to which the name giant may surely be applied. First of these is the huge Pirarucu, which is found in the rivers of Guiana and in the Amazon; then the great catfishes which inhabit practically all large South American rivers from Guiana to the Argentine. Since South America is the continent of catfishes, it is not surprising to find that one of these Siluroids attains such great size that it is called Goliath.

Our knowledge of the distribution and particularly of the sizes attained by these great fishes is very imperfect; as a matter of fact, very scanty. Extensive collections and considerable study have been made of the smaller South American fishes, since these can be taken fairly easily, can be preserved and can be transported to museums for description and classification. Most collectors are not likely to get the big fellows. Along these great rivers the population is thin, towns are generally small and far apart, and few persons, if any, are interested or able to catch, photograph, measure and weigh these great fishes. Opportunities for scientific men to do so are also very few, and large specimens, for obvious reasons, rarely come to museums.

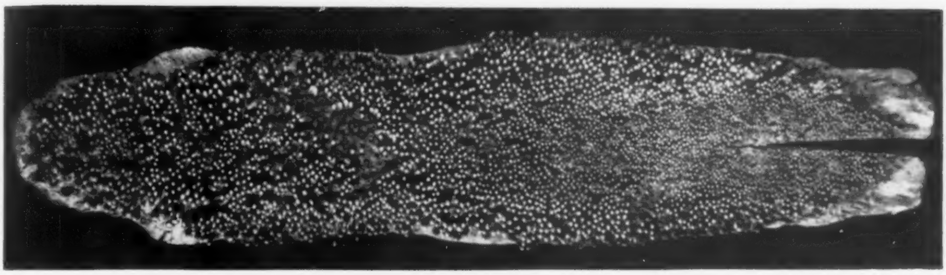
Charles H. Coles

FIG. 1. THE GIGANTIC OSTEOGLOSSID FISH, *ARAPAIMA GIGAS*

FOUND IN THE RIVERS OF GUIANA AND IN THE AMAZON. THIS MOUNTED SKIN IS 6 FT. 9 IN. LONG.

THE PIRARUCU, OR *Arapaima gigas*

This great fish is common to the Orinoco and other rivers of Guiana, and to the Amazon and its tributaries, but is not found in the La Plata or other south-flowing rivers. It has long been considered the largest fresh-water fish in the world, but, as has been and will be shown in other articles, this is an error. Its names are interesting; Pirarucu is the Indian name, from *pira*, fish, and *rucu*, red, because the reddish color of the great scales makes the whole fish look red, as may be seen in our mounted specimen in the Fish Hall of



Charles H. Coles

FIG. 2. THE BONY TONGUE (6.7 INCHES LONG) OF A PIRARUCU

THE TONGUE OF THIS FISH, BECAUSE OF ITS VERTICAL TEETH, IS THE UNIVERSAL GRATER OF AMAZONIA.

the American Museum. The size of the scales (if not their color) may be noted in a photograph of this specimen (Fig. 1). Arapaima is the Guiana Indian name, *gigas* is the Latin word meaning giant, and it surely fits our Arapaima. Furthermore, this fish belongs to the family Osteoglossidae (*osteon*, bone, and *glossa*, tongue), those fishes which have bony tongues. The tongue of our fish is covered with crowded rasp-like teeth. Indeed the inhabitants of the Amazon Valley use these tongues as graters to reduce to a pulp coconut meat, manioc and other fleshy roots.

This curious bony tongue apparently has never been figured and it seemed that this article would have to lack such an interesting illustration. However, Dr. Harvey Bassler of our museum staff, during a long residence at Iquitos on the upper Amazon, collected and sent to the Department of Ethnology of the American Museum three of these curious implements. They measure 3.6, 5.9, and 6.7 inches in length. The two larger have been used as graters. Both had the interstices between the teeth filled with organic matter which, when softened with warm water, could be cleared away with a needle and a stiff brush. The appearance of this curious natural implement is well brought out in Figure 2.

The flesh of the Pirarucu is very palatable, and when cut into strips, salted and dried, it plays in the Amazon basin the part of bacon in the Mississippi Valley. More narrowly, it may be said that, for the Amazonian riverines, it is the food counterpart of dried codfish in New England. The cutting up and curing of the flesh of this great food fish has been carefully illustrated and described by Professor W. R. Allen of the University of Kentucky. To his text and reproduced photographs, the inter-

After Eigenmann and Allen

FIG. 3. THE FLESH OF THE PIRARUCU

THE "BACON" OF THE AMAZON VALLEY. IT HAS BEEN CUT IN STRIPS, SALTED, DRIED AND BALED.

ested reader is referred.[1] One of his photographs, showing the cured meat tied up in bundles like slabs of bacon, is reproduced in Figure 3.

Arapaima gigas is gigantic, as its pictures show. The ichthyology books (and the dictionaries also) say that, "It reaches a length of fifteen feet, and a weight of 400 pounds," but no one gives his authority. All are like a flock of sheep—each following the one in front. The man who said it first was Robert H. Schomburgk. On page 201 of his *Fishes of British Guiana—Part I*, published in 1841, he says, "I was assured by the inhabitants of the Rio Negro that they had caught some fifteen feet long and of twelve or thirteen arrobas (410 pounds) weight." Schomburgk calls attention to the small size of the caudal fin. In a specimen eight feet one inch long, it was only five inches long and eight inches vertical when fully expanded. This small caudal fin is very noticeable in Figure 1 which was made from our mounted specimen in the museum, and also in Figure 4 which was copied from Franz Keller's book, *The Amazon and Madeira Rivers* (New York, 1874). From a study of these figures it is clear that the red fish must be a slow swimmer.

Schomburgk states that the Rupununi, one of the head-components of the Essequibo, was in his day the only river in Guiana known to him in which the Pirarucu had been taken. Interestingly enough, Figure 5, for which I am indebted to William Beebe and John Tee-Van, is of a fresh fish taken from the Rupununi. The photograph was obtained from a passing traveler and, unfortunately, no other data concerning it were available. It is, however, the only figure known to me of a freshly caught Arapaima. This photograph gives a very good idea not only of the size of the fish but of the shape and bulk. It is unfortunate that the fish was not suspended clear of the sandbar.

I have not been able to find just how large the Pirarucu grows. It is probable that Robert Schomburgk's fifteen feet in length may not have been an exaggeration for his day, but I feel quite sure that 410 pounds is too small a weight for a fifteen-foot fish of this heavy build. All the figures (drawings and photographs) that I have seen show the fish with a massive logy body (Fig. 4). The British Museum has stuffed skins seven and eight feet long; and a six-foot skeleton, all presented by Schomburgk. The larger of these specimens had a girth of forty-three inches. Our mounted skin measures six feet, nine inches over all. It came to us from Para at the mouth of the Amazon but there is no record where it was taken.

In 1909–1910, Dr. J. D. Haseman, under the auspices of the Carnegie Museum of Pittsburgh, made extensive collections of fishes on the upper Amazon, the Rio Negro, and their tributaries. He states in a letter that at Meura, on the Rio Negro, at the mouth of the Branco, he measured the eight-foot skeleton of an Arapaima, said by the native fisherman to have weighed 120 kilos (265 pounds). At the same place the fishermen had caught another Arapaima, which they claimed to have been ten feet long and to have weighed 200 kilos, or 440 pounds. Haseman did not see the fish but did see the head. This was so much larger than the head of the one he measured that he thought the report of length and weight "reasonably accurate." Haseman states that these powerful fish, each provided with a bony tongue and mouth, not only tear up the fishermen's nets but even chicken-wire enclosures set in the rivers.

Parenthetically, it may be remarked that this region of the Negro (at the

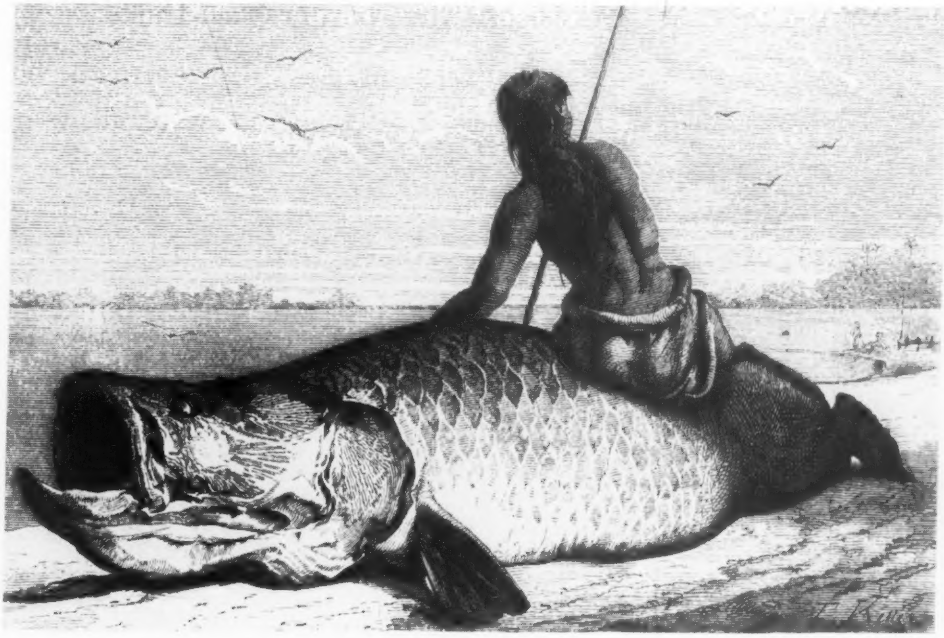
[1] C. H. Eigenmann and W. R. Allen, *Fishes of Western South America*, University of Kentucky, Lexington, 1942. (Natural history notes on Arapaima, pp. 336–345, 6 figs.).

mouth of the Branco) is well known as a special habitat of Arapaima. Robert Spruce, in 1868, speaking of his experiences there from 1850 to 1855—nearly 60 years before Haseman—says that: "About the mouth of the Rio Branco is the only [?] place in the Rio Negro where the Pirarucu is found—that noble and remarkable fish, so characteristic of the Amazon." If these various habitat notices are correct, there is much yet to be learned about the ecology of this fish.

The latest estimator of the size and weight of Pirarucu is Allen (1942, p. 338). The only one he measured taped seven feet, one inch, and his carefully considered judgment "would place the probable maximum size . . . , at least in the upper Amazon, at about twelve feet and the maximum weight . . . [at] 300 pounds." But Allen notes that due to overfishing the numbers of Arapaima are steadily decreasing. The day of the big fellows would seem to be over.

Haseman reports an interesting habit of the red fish on the Branco as follows: "About sunset these fishes come to the surface and make a tremendous explosive noise that can be heard over a quarter of a mile." The natives say that this is to get air, but Haseman concluded that it was made by a strong slap of the tail on the surface of the water. He offered no conjecture why this is done.

This great fish's interesting method of reproduction is imperfectly known. Its huge mouth, shown particularly well in Keller's figure, must be connected with its food and feeding habits and very probably with its method of reproduction. Of the former we know practically nothing, but on the latter Schomburgk throws some light. "The young are protected by the mother for some time after they leave the eggs [*i.e.*, hatch] just as in the case of the lau-lau [a giant catfish next to be studied], and swim generally



After Keller, 1874

FIG. 4. A PIRARUCU FROM THE AMAZON RIVER

PARTICULARLY NOTICEABLE IS THE CAVERNOUS MOUTH IN WHICH THE YOUNG TAKE REFUGE.

William Beebe and John Tee-Van

FIG. 5. A PIRARUCU FROM THE RUPUNUNI RIVER, BRITISH GUIANA

IT IS LONGER THAN EITHER MAN AND APPEARS TO BE ABOUT AS HEAVY AS BOTH MEN TOGETHER.

over her head." Oral gestation or buccal incubation (the carrying and holding of the eggs in the mouth of one parent or the other until they hatch) is definitely known to be practiced by two other Osteoglossids—one in the Amazon and the other in the rivers of Borneo. There is an extensive literature from 1768 to 1912 which alleges parental care by the Pirarucu and a number of the writers affirm that the eggs are carried in the gills. I have carefully studied all these accounts and find that they overwhelmingly point to oral gestation. The sex has not been determined in any of these Osteoglossid egg-carriers.

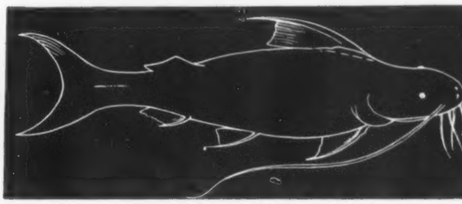
All accounts state that these great fishes are taken with the harpoon or with bows and arrows, rarely with the hook, and almost never in nets. Taken thus, the fish struggles violently and the eggs or young are likely to be disgorged. But perhaps some day some fortunate observer will by examination or by dissection find the eggs in the buccal cavity and determine the sex of the carrier.

The Great Catfishes of South America

All the great rivers of South America are inhabited by huge catfishes. They are large and unwieldy and are com-

monly taken by fishermen lacking not only cameras for making pictures but also appliances for measuring and weighing these giants. Consequently the information we have received is very sketchy. Furthermore, scientists are rarely around when these huge fishes are taken, and because of their great size adult catfishes seldom come to museums.

The Lau-Lau of the Guianas and the Amazon. This great siluroid, whose scientific name is ***Brachyplatystoma filamentosum***, is found in the Guiana rivers and also in the Amazon. It and others of the genus are notable not only for great size but also for their long wide heads and huge gaping mouths—as our illustrations show. Notable also are their prodigiously elongated filaments—the barbels or "whiskers" around the mouth which give these fishes the name "catfish." Lau-lau is the Guiana Indian name, which seems to have been first recorded by William Hillhouse in 1825 and by Robert Schomburgk in 1841. Hillhouse appears to have been the first to publish on the natural history of the Lau-lau. In his book, *Indian Notices . . . also the Ichthyology of the Fresh Waters of . . . British Guiana* (Demerara, 1825), he says that the Lau-lau attains a length of twelve feet and a weight of 250 pounds.



After Robert Schomburgk, 1841

FIG. 6. SKETCH OF THE LAU-LAU

EARLIEST KNOWN FIGURE OF THIS GREAT FISH.

Robert Schomburgk gives us further natural history notes on this big siluroid. He writes (1841) that: "The

William Beebe and John Tee-Van

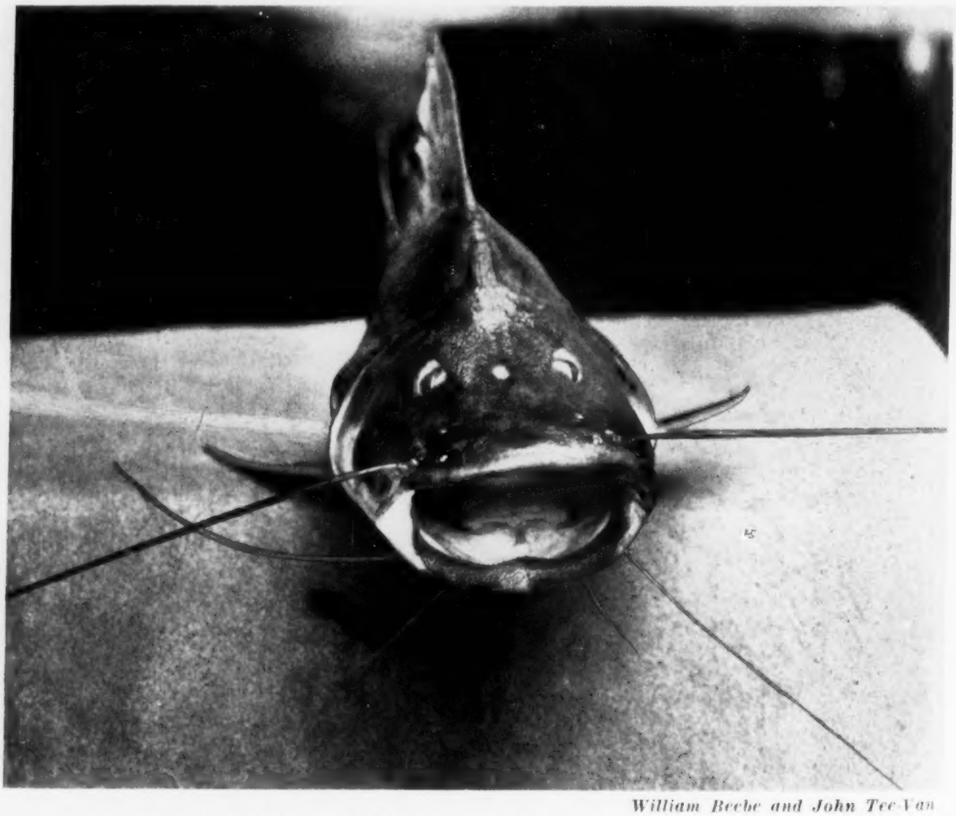
FIG. 7. THE LAU-LAU, LARGEST CATFISH FOUND IN BRITISH GUIANA

THE GIANT OF ALL FISHES FOUND AT KARTABO. NOTE THE BROAD HEAD AND THE WIDE MOUTH.

Lau-lau is, next to the Pirarucu (*Sudis gigas*), the largest fresh-water fish in the rivers of Guiana. . . . They sometimes attain the length of ten or twelve feet, and the weight of 200 pounds, and their flesh is much esteemed." Their chief food is fishes, though they tend to be omnivorous. Their strength is in proportion to their size and they are strong swimmers. Illustrative of these points, Schomburgk tells the following story, which also indicates how the fishes are caught.

> Sororeng, one of the Indians . . . went late in the evening alone in a canoe, to try whether he could hook some fish. We were all fast asleep, when I was awakened by some person crying out for help, and we soon ascertained that it was Sororeng, who had hooked a Lau-lau and having got entangeled in the line, with neither knife nor other sharp instrument at hand, the fish carried him and canoe at a rapid rate toward the cataract. Armed with cutlasses, we soon came to his assistance, and in time enough to prevent him from being carried down; but so eager was he now to secure his prize, when he saw that assistance was at hand, that he begged us not to cut the line, although it had, by this time, fairly cut into his hand, but to try to slay the monster, which apparently was more inclined to kill the fisherman than the fisherman the fish. It was slain and when brought to land, measured eight feet and a half in length.

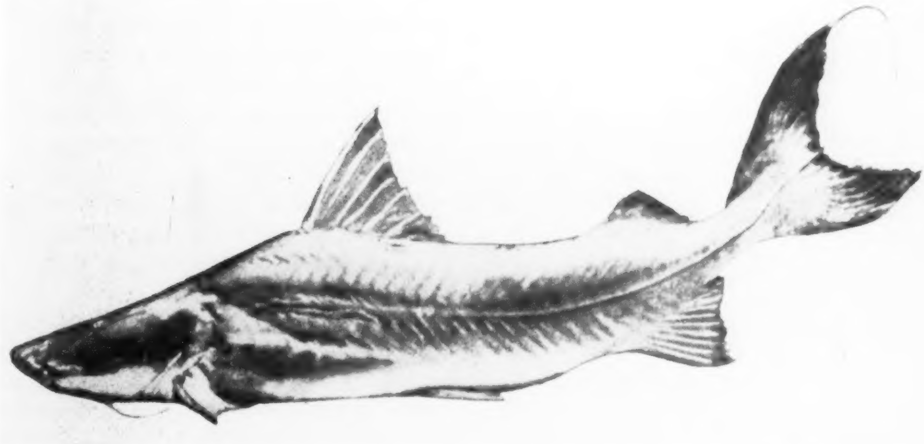
A good figure of this great catfish is greatly desired. Robert Schomburgk had with him an artist who made colored drawings of selected fishes, but for the Lau-lau he got no further than a mere outline sketch in pencil (Fig. 6). This is the earliest representation of this huge



William Beebe and John Tee-Van

FIG. 8. THE CAVERNOUS MOUTH OF THE CUMA-CUMA, GUIANA CATFISH

ALTHOUGH IT IS MUCH SMALLER THAN THE LAU-LAU, THE MOUTH COULD HOLD A PECK MEASURE.



After Goeldi, 1911

FIG. 9. THE PIRAHYBA, A GIANT CATFISH OF THE AMAZON

CLOSELY RELATED TO THE LAU-LAU. ITS NAME MEANS LARGE FISH. THIS ILLUSTRATION PRESUMABLY WAS MADE FROM THE SEVENTY-EIGHT-INCH MOUNTED FISH IN THE PARA MUSEUM.

fish, but even in outline it brings out the characteristics noted above.

Robert Schomburgk's material came from the junction of the Rupununi and Essequibo Rivers, October 15, 1837. Ninety years later William Beebe and John Tee-Van studied specimens taken at Kartabo at the junction of the Cuyuni and Mazaruni Rivers, just six miles above where their united waters flow into the Essequibo. I am privileged to quote from their unpublished manuscript and to reproduce an unpublished photograph of this great catfish (Fig. 7). They state that this is "the giant of all the fishes found at Kartabo." The Indians told them of specimens twelve to fourteen feet long, and they found six-foot specimens to be fairly common.

The Lau-laus were caught at night on set lines with baited hooks sunk to the bottom in midstream. Unlike Sororeng's fish, they offered little resistance and when a Lau-lau is caught ". . . the first evidence of its presence in the discolored waters is to see one sliding over the gunwale of the boat [in which it is brought ashore]. And when hauled into a boat, they are quite apathetic and seldom thresh about any." The passivity of these specimens may be attributed to the fact that, caught on set lines in the night, they have probably struggled for hours in the endeavor to escape and have literally become worn out.

The two largest Lau-laus taken by Beebe and Tee-Van measured seventy-four and seventy-five and a half inches and weighed two hundred and two hundred and ten pounds respectively. These measurements were from tips of snouts to bases of caudal fins, not "over-all" lengths, which, for our purpose here, are preferable. The tail fin of a large fish would add about a foot to the standard length of the fish. In 1916, Beebe and Hartley measured a Lau-lau, taken near Kartabo, which stretched for eighty-three inches from tip of snout to base of tail, or about ninety-five inches (eight feet) over all.

The flesh of most huge fishes is apt to be rank, "strong," but that of the Lau-lau is very palatable despite its size. The capture of a big fellow is quite a gastronomic event and its flesh is peddled up and down the river to the no small satisfaction of the riverines.

As to the Lau-lau's method of reproduction, the evidence (as in the case of the Arapaima) points strongly to oral gestation. Our first intimation of this comes from Hillhouse, who states that the young "swim in a school of 50 to 150 just over the head of the mother. On the least alarm, the mother opens her mouth, and the whole [school of] fry rush into the thorax and stomach [mouth and pharynx] from whence they are not again returned till the appearance of danger is over."

Many years ago, while studying oral gestation in the gaff-topsail catfish (*Felichthys felis*), I went minutely through the books on Guiana fishes and their habits. From this study the evidence is convincing that the Lau-lau and some seventeen other species of Guiana catfishes practice oral gestation. If so, then the *males* (in all known catfishes), *not the females*, carry the eggs in their mouths until they are hatched. And the little fishes stay therein until they are able to seek their own living. Even then they swim around the head of the father and take refuge in his capacious mouth when danger threatens. This habit, which is widespread among fishes practicing oral gestation, is identical with that noted above for the Pirarucu.

In the marine gaff-topsail catfish (which practices oral gestation) the eggs are large, averaging about twenty millimeters ($\frac{3}{4}$ inch) in diameter, and numerous, up to fifty-five having been taken from the mouth of one male. To accommodate them an enormous mouth cavity is needed. All catfishes have disproportionately large heads (Fig. 8) and if the Lau-lau is to accommodate 150 just hatched young, each probably at least five inches long, it must have a huge mouth. There is no "head on" picture of this fish, only one of the closely related Cuma-cuma (*Brachyplatystoma vaillanti*). This photograph (for which I am indebted to Beebe and Tee-Van) is of the head of a specimen about thirty inches long (Fig. 6). Large as is the mouth of this thirty-inch Cuma-cuma, how much larger would be that of a seventy-five inch Lau-lau. One wonders if other great South American catfishes with huge mouths also practice oral gestation; probably at least some forms closely related to the Lau-lau do so.

The Pirahyba of Amazonian Waters. Our great catfish (the Lau-lau) is found in the Amazon and its tributaries. This is not unexpected since it is a powerful swimmer and since the Orinoco and the Rio Negro are connected by the Cassiquiare. The Lau-lau (*Brachyplatystoma filamentosum*) has never been clearly differentiated from other and closely related forms, particularly those belonging to the same genus. Even the Brazilian ichthyologists have not had much success in separating them. The adults are huge, only the smaller and immature specimens are found in collections, hence the difficulty in naming them correctly. Furthermore, few writers are able to give sizes and weights. The most promising articles on these great catfishes are by the Brazilian ichthyologists, E. A. Goeldi and Miranda Ribeiro. Goeldi's article bears the intriguing title: "A Piraiba: Gigantesco Siluroideo do Amazonas; Piratinga piraaïba = Piratinga filamentosa."

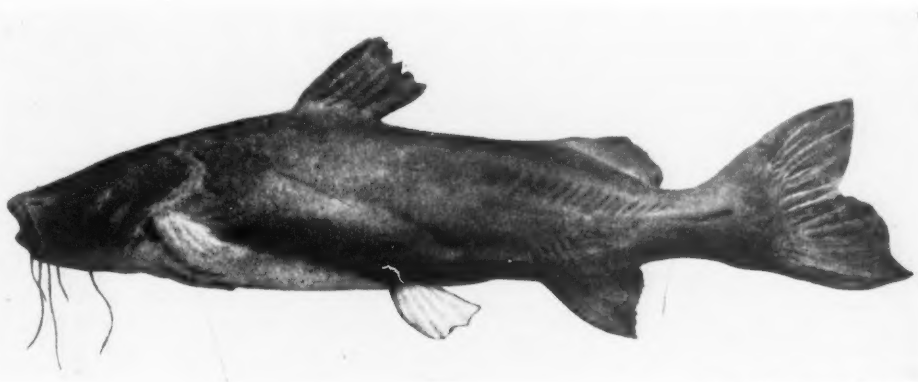
Piraiba, Pirahyba, and Piratinga are the common names by which these huge Siluroids are known on the Amazon and its tributaries. The first two names are evidently of Tupi Indian origin. *Pira* means fish and *aiba* or *hyba* means large —large fish, and this name is probably applied to any large Amazonian catfish. Professor Eigenmann recognized seven species of Pirahyba.

Little of the natural history of this big catfish can be learned from the articles by Goeldi and Ribeiro, which are mainly

concerned with classification made from a study of comparatively small specimens. This classification is very difficult since the fishes seem to have been imperfectly described and given many names. However, Goeldi speaks of another colossal and popular Pirahyba which he calls the Goliath of catfishes but for which he gives no measurements. He has a figure of a Pirahyba which he says was made from a two-meter fish (Fig. 9). It looks as if it were made from a dried specimen, possibly from the mounted specimen in the Para Museum. This was seventy-eight inches long and thirty-nine and a half inches in girth at the front base of the dorsal fin. No weight is given.

In this dearth of published natural history data on the Pirahyba, it is a satisfaction to be able to quote from a recent letter by Dr. Haseman, who was on the upper Amazon tributaries in 1909–1910 collecting fishes for the Carnegie Museum. In the Guapore branch of the Mamore River, he caught on a big hook baited with a piece of fat bacon a Pirahyba so large that it took four men to drag it out on to a sandbar. Its weight was estimated at three hundred and fifty pounds. As to its length:

It was not measured by tape but, after the four of us dragged it onto a sandbar, we stood it on its head and its caudal fin reached about one inch above my head—and I stood six feet high. Its huge head had to be cut off with an axe and was about all that a strong man could lift. This head measured fully one-third the length of the fish—not counting the tail. The mouth was amply large enough for a small Brazilian to crawl through. Indeed it is claimed that the Pirahyba does swallow swimming natives but I was unable to confirm this. How big the fish grows no one knows. I hooked one in a whirlpool in one of the Rio Madeira falls so big that two of us could not hold it. It broke the big trot lines which we had tied to a boulder, and carried off our big hook. It was without doubt larger than the Rio Guapore fish. Six to seven feet and 300 to 400 pounds are in my opinion about the limit for the Pirahyba. It is truly a giant fish.



After MacDonagh, 1937

FIG. 10. THE BLACK MANGURUYU OF THE ALTO PARANA

NEITHER MEASUREMENT NOR WEIGHT WAS GIVEN FOR THIS SPECIMEN. THE BLACK MANGURUYU PROBABLY GROWS AS LARGE AS THE LAU-LAU OR THE PIRAHYBA. IT IS A POWERFUL SWIMMER.

How one wishes for a photograph of this six-foot Pirahyba with its captor standing beside it! Dr. Haseman's exploring trip in South America was an outstanding one; his collections (necessarily of smaller fishes) were very many and very valuable, but his priceless negatives were all spoiled by heat and humidity—an irreparable loss to ichthyology.

The report that a large Pirahyba can swallow a boy or even a small man is corroborated by President Theodore Roosevelt in his *Through the Brazilian*

Wilderness (New York, 1914, pp. 311–312).

> The huge catfish which the men had caught, . . . with the usual enormous head, out of all proportion to the body, and the enormous mouth, out of all proportion to the head. Such fish . . . swallow very large prey. This one contained the nearly digested remains of a monkey, and . . . once engulfed in that yawning cavern there was no escape. . . . our Brazilian friends told us that in the lower Madeira and part of the Amazon near its mouth, there is a still more gigantic catfish which in similar fashion occasionally makes prey of man.

This fish, called Piraiba, was said to grow over nine feet long. While stationed at Itacoatira, a town on the Amazon at the mouth of the Madeira, the expedition's doctor saw one measuring nine feet. This fish had been killed with machetes by two men, when its head rose out of the water alongside the boat from which they were fishing. Swimmers are said to fear these big catfish more than they do the big caymans or crocodiles. Colonel Rondon stated that on the lower Madeira, because of fear of the Piraiba, the villagers built stockades around their bathing places.

The Mysterious Manguruyu, or Jahu, of the La Plata and Tributaries. These names are given to a very large catfish found in the La Plata drainage. There are several forms which belong to the genus *Paulicea*. Possibly the best known one is *P. lutkeni*. This particular fish has an extraordinary distribution—from the shores of Guiana to the mouth of the La Plata. It has been recorded from the rivers of Guiana by Beebe and Tee-Van, and by others. Brazilian ichthyologists have taken it in the Amazon. Haseman found it, under the native name "Jahu," in the headwaters of both the Madeira and the Paraguay-La Plata rivers. That it is common to the Guiana and Amazon waters is easy to understand since the Cassiquiare links the Orinoco and the Amazon. But how it got into the Paraguay-Parana-La Plata waters is a great puzzle.

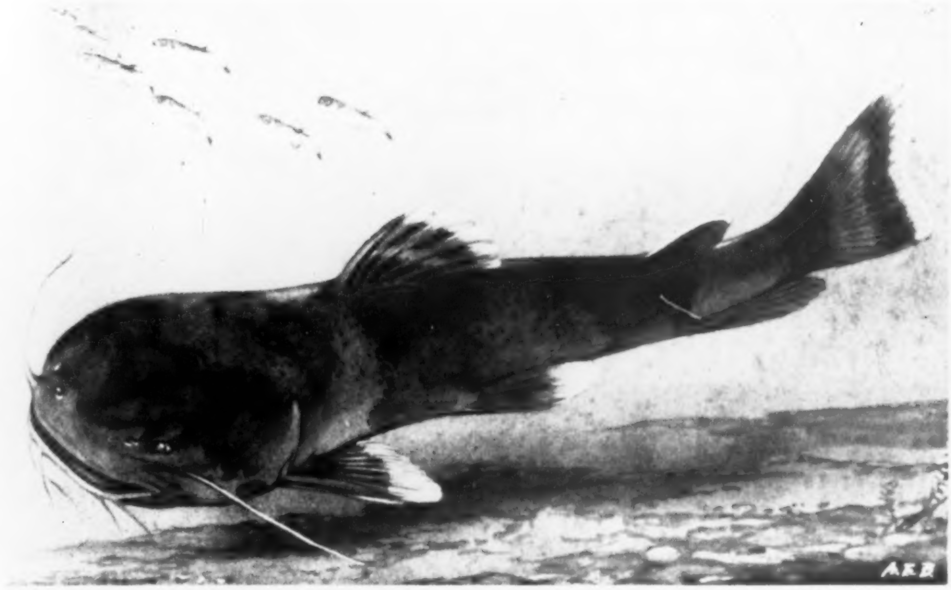
The fact that this great siluroid and scores of other kinds of fishes are common to both Amazon and La Plata tributaries is strong evidence that one or several connections formerly existed between these river systems. I have seen maps which show the headwaters of one of the great southeastern tributaries of the Amazon arising in a swamp on a flat divide in Central Brazil, while out of the other side of the same marsh flowed a stream entering into the headwaters of the Parana. Reclus, the great geographer, states that in 1772 a canal was cut through the narrow isthmus (about half a mile wide) separating the headwaters of the Guapore-Mamore from those of the Paraguay. Furthermore, it has been stated that when these headwaters are in flood the flat divides are under water. This matter is a most interesting side issue of the study of these great fishes. Here is a field for a fascinating piece of geographical research.

The Manguruyu is the largest fish found in the La Plata system. Little, unfortunately, is know about its natural history and that comes mainly from the Alto Parana branch of the La Plata. It has not been possible to find the origin of this native name of the fish.

Emiliano MacDonagh[2] of the La Plata Museum has studied the Manguruyu negro, (*Paulicea lutkeni*), of which he had three fairly large specimens. His largest fish measured sixty-seven inches. His illustration made from the plaster cast in the Museum is reproduced here, since it is probably the best figure available of this large fish (Fig. 10). It shows the Manguruyu to be a very heavily built and evidently powerful fish.

C. H. Cuthbert, Honorary Editor of the Dorado Club *Guide*, Buenos Aires.

[2] "Sobre el Manguruyu (Genero Paulicea, Siluroideos)," *Revista Museo de La Plata*, 1937, new series, vol. 1, pp. 3–30, 14 figs.



After McCormick, 1937

FIG. 11. THE MYSTERIOUS MANGURUYU
FROM A DRAWING BY A. FRASER BRUNNER MADE FROM A SMALL SPECIMEN IN THE BRITISH MUSEUM.

writes that "The Manguruyu is the largest of the catfishes in the La Plata system of rivers, and cases have been recorded of specimens being captured of over 300 pounds."

Some years ago, L. J. McCormick of Chicago visited Guayra on the Alto Parana and fished below the falls. He writes interestingly of his experiences.[3] Warned that the principal fishes there were so large and heavy that they would smash everything but the heaviest tackle, Mr. McCormick, procured some heavy cord, tested to break dry at ninety-one pounds dead weight, and put on huge hooks baited with large pieces of raw meat. One man got a bite but, powerful as he was, could not hold his fish. Another man came to his help but the fish took the line from them both, cutting their hands badly in the process. Other lines fastened to boulders were broken. Another was tied to a large tree limb as a float, but a fish took the bait and pulled the limb under water—almost doing the same thing with the fisherman—and then broke the line. There was no holding these huge and powerful fishes. "The whole *remanso* [a quiet backwater] seemed alive with monsters . . . [but] the smooth surface of the turbid water gave no indication that anything stirred below." But the great fishes simply took the hooks, broke the heavy lines and went quietly about their business. This account recalls what Dr. Haseman wrote of his experience in hooking Pirahybas in the Amazon headwaters.

Having with him his swordfishing harness, rod and reel, and hooks and line, McCormick made one more effort. He got into his harness, boarded a canoe, baited a big hook, and fished swordfish-fashion. A fish took the bait and hook and, despite the heavy reel with the drag on, it took also about 200 feet of line. "I wanted to play the fish, but at about [a speed of] five knots, it simply went cruising along in an unconcerned and exasperating manner." From which it seems that the fish reversed the usual role and

[3] *Fishing Round the World*, New York, 1937; Chapter 4, "The Mysterious Manguruyu."

played the angler. The line then became fouled under water and, in attempting to loosen it, broke and the fish was lost. McCormick concludes: "My advice to those fortunate enough to voyage up the Alto Parana is to fish for Manguruyu—but to use a rope."

Although these great fishes at this very locality sometimes come to and roll at the surface, all of McCormick's fishes stayed at the bottom and, since they were never seen, no positive identification was possible—hence the title of his chapter on this fish. However, the native fishermen believed that they were Manguruyus since there were in those waters no other fishes large and powerful enough to break such heavy lines. In this conclusion all McCormick's South American friends and all ichthyologists consulted agree.

To illustrate this huge fish, McCormick, on the advice of J. R. Norman of the British Museum, had a drawing made of a small specimen of Manguruyu in that museum by the well-known English animal artist, A. Fraser Brunner (Fig. 11). The angle at which the drawing presents the head gives some idea of the width of mouth and head but not of the length of head.

How large this great catfish grows is not known. The Dorado Club in Buenos Aires in its *Guide* for 1937 has unverified reports of very large ones. One measured sixty-six and one half inches in length, forty and one half inches girth, and the head length thirty-one inches—nearly half the length of the fish. Its weight was not taken but it was estimated at about 200 pounds. Another, when caught and pulled to the surface, was so huge that a horse was required to drag it ashore. It was reported to be seven feet long and to weigh 220 pounds. A third when hooked pulled the fisherman out of his canoe but he hung on and was rescued by his friends. Then the fish took charge of the canoe and towed it down stream about seven miles and was subdued only after a three-hour fight. No length is given but the fish is said to have weighed 308 pounds. Next, Sir Christopher Gibson reported having found, left stranded by a receding flood, "a Manguruyu skull which was over 6 feet in length." The fish was judged to have been thirteen or fourteen feet long and to have weighed about 660 pounds. In support of this, it is said that "the naturalist Ambrosetti mentions obtaining specimens over 4 metres (14 feet, 6 inches) from the river near the town of Parana."

These statements are all made by non-scientific men: Sir Christopher Gibson does not say that he put the tape on his huge skull, and I have not been able to locate and identify "the naturalist Ambrosetti"—consequently they must be taken for what these accounts are worth. But as the late Dr. F. A. Lucas wrote, "All fishes shrink under the tape." Hoping to get authentification for these accounts, I wrote to Cuthbert but he could not get any further data. However, he wrote to Sir Christopher and sent me a copy of his answer. I quote from Sir Christopher's letter—italics and all.

> The skull I saw . . . *was* 6 feet in length; but . . . at the time—so long ago—I never realized the importance of preserving such a valuable specimen. However, I *believe* I am right in saying that the total length of the skull is greater than half the total length of the fish, which would make this particular fellow, say 11 feet long or . . . say 250 kilos [550 pounds].

Undoubtedly Sir Christopher saw a huge catfish skull, but he did not measure and preserve this unique specimen. This is most unfortunate and regrettable. Had he done so there would be something tangible whereon to estimate the length of the fish. The length of a fish is reckoned thus—Head in body x times, and the "head" is measured from snout or mouth tip to tip of bony gill-cover. But

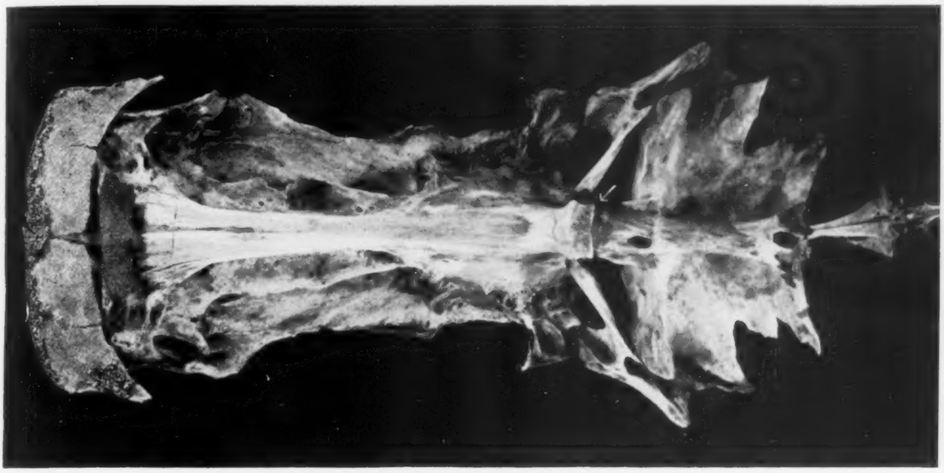


FIG. 12. "SKULL" OF A GIANT CATFISH
THE *Brachyplatystoma filamentosum* FROM BRITISH GUIANA. THE SKULL PROPER ENDS AT THE JUNCTION INDICATED BY THE ARROW. TO THE RIGHT OF THIS IS THE "VERTEBRAL COMPLEX."

when the head is macerated, and the loose bones (gill-covers, jaws, etc.) fall away, the solid bony skull is left. This is generally somewhat shorter than the head as defined above, but not always so. However, the "skulls" of many catfishes are extraordinary structures—much longer than the head as defined.

This is seen in Figure 12. Here the base of the skull proper is marked by the arrow and to the right are several vertebrae and their transverse processes solidly fused into a unit mass called the "complex vertebra." This is so firmly joined to the base of the true skull as to be essentially a part of it. The number of these vertebrae varies from five to eight in various catfishes. The skull figured here in ventral aspect is from one of Dr. William Beebe's huge British-Guiana catfishes—*Brachyplatystoma filamentosum.* Here seven and possibly eight vertebrae and their lateral processes are fused to make the vertebral complex.

In the specimen figured, the complex vertebra greatly elongates the "skull" as in that seen by Sir Christopher Gibson. The total over-all length of the skull as portrayed (Fig. 12) is 472 mm. (18.6 in.) but of this the vertebral complex measured from the base of the skull backward is 126 mm. (4.95 in.). Then the length of the true skull is 346 mm. (13.6 in.) and it is clear that the complex vertebra comprises or adds 36 per cent. to the length of the "skull" in Sir Christopher's phraseology. If the same proportions hold true in the skull seen by him, then the true skull must have measured less than four feet long and the length of the fish that bore it must have on his calculation ("body = head × 2") been about eight feet in length.

Furthermore in this matter of head-length body-length proportions in the Manguruyu, we have positive evidence from MacDonagh. He had in the La Plata Museum three fairly large specimens and for these he gives the following proportions: (1) in a fish 1455 mm. long, the head was contained in the body 3.43 times; (2) 1555 mm. long, and head length in the body 3.57 times; and the proportion for (3) one 1680 mm. long was 3.33 times. None of these proportions approach the fish of 68.5 in. with a reputed head length of 31 in. or 45 per cent. of total length. These last measurements were probably erroneous.

EXPLOSIVES—VERSATILE TOOL OF INDUSTRY AND WAR

By Dr. JAS. K. HUNT

E. I. DU PONT DE NEMOURS AND COMPANY

RECENT announcement by Major General L. H. Campbell, Jr., Chief of Army Ordnance, of a new explosive said to be 35 per cent. more powerful than TNT (trinitrotoluene), has stimulated fresh interest in the whys and wherefores of explosive materials. What is an explosive, what is the source of its power, what causes it to "go off," and how is its energy measured? Do explosives have important uses other than in guns, bombs, torpedoes, and other military devices?

Strange as it may seem to those who think of explosives largely in connection with war, materials of this type are used chiefly for peaceful jobs. Commercial explosives, such as dynamite, blast out coal, ores and rock, and speed the contruction of dams, tunnels, highways and harbors. For such jobs, in a normal year, the United States consumes some 350,000,000 pounds of dynamite. The total this year may reach 450,000,000 pounds. In laying the "Big Inch," the world's largest pipeline, to bring oil from Texas to the East, approximately 750,000 pounds of dynamite were used. From scores of plants throughout the country, this versatile product is rolling to the mines to speed production of vital metals and coal.

Dynamite unquestionably ranks among the great labor-saving "tools" of all time. With fifteen cents worth of this explosive, and another fifteen cents worth of drilling, some four to six tons of rock may be put into useful form. History records that 100,000 slaves labored thirty years to build the great pyramid of Giseh. Grand Coulee Dam, a structure three times as large, was built in a fraction of the time and with a fraction of the workers—with the aid of dynamite. But, like other useful tools, explosives may destroy as well as build, and in time of war military explosives, such as TNT and smokeless powder, take the spotlight.

No simple definition would fit every explosive. All of them, however, are substances which, suitably stimulated, decompose with violence. That is, they go off with a "bang." Chemically speaking, the extremely rapid reaction that occurs when an explosive goes off involves the breaking down of one relatively large molecule into a number of rather small molecules. In the case of most common explosives, the decomposition products are chiefly molecules of such simple gases as carbon dioxide, carbon monoxide, nitrogen, hydrogen, and water vapor. Some explosives, however, form no gases when they go off. In the explosion of copper acetylide, for example, the decomposition products are carbon and copper—both solids. There is little or no recombination of the simple decomposition products—either gaseous or solid—into more complex molecules.

The stimulus required to set off explosives varies widely. Compositions based largely on ammonium nitrate, unless sensitized with nitroglycerin or similar material, are so insensitive to shock as to require special primers or detonators. For example, the ultra-safe "Nitramon" blasting agent, employed chiefly in rock quarrying, uses a primer based on TNT. An ordinary dynamite blasting cap does not provide sufficient "kick." TNT itself is capable of with-

Courtesy Du Pont Company

BLASTING THE TRENCH FOR "BIG INCH"

ONE OF SEVERAL BLASTS MADE TO CREATE THE RIVER-BED TRENCH FOR "BIG INCH" IN RECORD TIME.

standing a severe shock without exploding. Otherwise it could not be used as the bursting charge in shells shot from big guns. Other explosives, such as nitrogen iodide, may be set off by the gentle touch of a feather.

The use to which an explosive is put depends in part upon its sensitivity to shock. This can perhaps best be illustrated by considering the several different materials used in high-explosive shells, such as those fired from large-caliber naval guns. The explosive train in the simplest type of high-explosive shell consists of three materials: the primer, the "booster," and the main bursting charge.

For the primer, which is located in

Courtesy Du Pont Company

LOADING A 25 LB. CARTRIDGE OF 60 PER CENT. GELATIN DYNAMITE

THE CARTRIDGES WERE LOWERED THROUGH STEEL CASINGS INTO DRILL HOLES AND FIRED TO BLAST A DITCH IN THE RIVER BED FOR "BIG INCH," THE WORLD'S LARGEST OIL PIPELINE.

the nose of the shell, a small charge of a rather sensitive explosive is used, such as lead azide or fulminate of mercury. This primer charge is set off by percussion when the shell strikes something solid, or by a timing mechanism as is employed in some anti-aircraft shells which are set to explode at any desired altitude. Although the material used as a primer must be sensitive enough to be set off when the shell strikes a solid object, it must be capable of withstanding a certain amount of shock; otherwise, accidental explosions would be frequent.

Next in the explosive train of the shell may come the "booster" charge, trinitrophenylmethylnitramine ("tetryl") being typical of this class of explosive. "Boosters" are less sensitive than the primer, but more sensitive than the HE (high explosive) bursting charge. The "booster" makes it necessary to use only a small amount of sensitive and relatively dangerous primer, and insures complete detonation of the relatively insensitive bursting charge—usually TNT, Amatol (TNT and ammonium nitrate), or, for armor-piercing shells, ammonium picrate. Considerably more "booster" is used than primer, but even then the "booster" charge is small in comparison with the HE bursting charge. It is the charge of HE which does the damage in the so-called "block-busters."

Frequently a time fuse is used to delay detonation of the bursting charge until, for example, the shell has passed through the armor plate of a battleship—a sheet of steel twelve inches or more in thickness. Slow-burning black powder is commonly used in such time fuses, the effective length of which is controlled by a dial setting on the nose of the shell. When a black-powder fuse is employed

to delay detonation, the train of explosives in the shell usually consists of primer, time fuse, detonator, "booster," and HE bursting charge.

The detonator used in such an explosive train may be identical in composition with the primer, and is necessary because the black-powder fuse alone would not set off the relatively insensitive "booster." The explosives used as primers and detonators, although not necessarily extremely powerful (as compared with certain other explosives), are materials which deliver their energy rapidly and at extremely high pressure. In fact, some detonators develop pressures of the order of two and one-half or three million pounds per square inch.

All explosives are decomposed by heat, but with some—dynamite, for example—a rather severe shock is generally necessary to cause the instantaneous and violent decomposition known as "detonation." To bring about the necessary shock a metal cap containing a suitable detonator is attached to the dynamite. This cap may be fired either by a black-powder fuse or by an electric current.

Large volumes of gases are set free when most explosives go off. Furthermore, the temperature of an explosion may be 5,000 degrees Fahrenheit, or over, which greatly expands the liberated gases. As a result, the volume of hot gases formed may be from 10,000 to 15,000 times the volume of the exploded material. It is the large volume of gas liberated when smokeless powder burns that "propels" the shell out of a big gun and sends it on its deadly mission, possibly twenty-five miles away. The pressure of this gas, incidentally, rises sharply to a maximum—around 36,000 pounds per square inch in a sixteen-inch

Courtesy Du Pont Company

UNLOADING "BIG INCH" PIPE BESIDE THE SUSQUEHANNA AT MARIETTA, PA.

ABOUT SIXTY THOUSAND POUNDS OF 60 PER CENT. GELATIN DYNAMITE WERE USED TO BLAST THE TRENCH IN THE SOLID ROCK BED OF THE RIVER BOTTOM IN WHICH THE BIG PIPE RESTS.

gun—before the projectile has traveled one-fourth the distance from breech to muzzle, after which it falls off gradually.

The energy of an explosive depends upon several factors, including the volume of gases produced and the amount of heat given off. It is commonly determined by shooting a definite amount of the explosive in a "ballistic mortar," a device somewhat like a heavy swinging cannon, and by observing how much swing is imparted to the mortar by the "kick" of the explosion. The greater the swing, the greater the energy. The energy or "strength" of an explosive is commonly expressed in terms of TNT, the standard of comparison in the United States. Equally important as *amount* of energy, however, is the *rate* at which the energy is liberated, since this determines the usefulness of explosives for various purposes.

Explosives are divided into two main types, propellants and high explosives, depending upon the speed at which they burn or decompose. The faster the burning, the faster energy is released. In turn, rate of burning depends upon such factors as particle size, density, and pressure.

Black powder, first used as a propellant by the English in 1346 in the battle of Crécy, burns at a rate of only a fraction of an inch per second in the open air. Confined, however, so that a high pressure is built up, the rate of burning may be ten or more times as great. The rate of combustion of smokeless powder likewise increases with pressure, but in no case—in a sixteen-inch gun, for example—does the average rate exceed three or four inches per second. In contrast, the velocity of detonation of a high explosive, such as dynamite or TNT, may exceed 20,000 feet a second.

The burning rate of smokeless powder is measured by firing a weighed charge in a closed chamber and measuring the rate of pressure rise with a piezo-electric gauge—a device which converts pressure on a quartz plate into an electrical impulse. Electronic amplifiers and a cathode ray oscillograph translate the electrical impulse into a visible trace which may be recorded photographically.

With this technique the burning characteristics of numerous propellants have been examined. The observations are consistent with the theory that smokeless powder burns only on the surface, that the decomposition takes place rapidly and completely in the neighborhood of the surface, and that the rate of burning—and accordingly the rate of gas evolution—is essentially proportional to the first power of the pressure of the gaseous products. In other words, if the pressure is doubled, the rate of burning is about twice as great.

Nitrocellulose-base cannon powders, such as those used by the U. S. Army and Navy, burn with a linear velocity of approximately one-fifth of an inch per second per thousand pounds of pressure per square inch. At 20,000 pounds per square inch, which may be taken as the average pressure in a modern field piece, the linear rate of burning is accordingly about twenty times as great, or around four inches per second.

Since the total time, from the firing of the primer to the time the projectile leaves the gun, is only a few hundredths of a second, even for the largest gun, it is evident that the least dimension of the powder grains must be quite small to insure fairly complete combustion before the projectile reaches the muzzle of the gun. The fact that the rate of gas evolution is proportional to the area of powder surface makes careful control of the burning surface essential in order to keep the gun pressures within the bounds of safety, which is of the order of 40,000 pounds per square inch. During manufacture not only must the chemical composition of a propellant be held constant to insure a product of uniform potential

energy, but the linear dimensions of the grains must be held within close tolerances to furnish material with the desired specific surface. These precautions are necessary in order to insure ballistic properties as nearly uniform as possible.

The ordinary present-day cannon powder liberates approximately twenty-eight cubic feet of hot gas (measured at atmospheric pressure) per second from each square inch of powder surface when burning under a pressure of 20,000 pounds per square inch. At greater pressures the rate is proportionately higher.

Because rate of burning depends on the size and shape of the powder grain, it is only natural that a great variety of geometrical forms has been tried. Strips, tubes, cubes, flakes, cords, spheres and multiperforated cylinders are some of the shapes which have been produced for various services. A short cylinder, with seven perforations parallel to the axis, is preferred in this country for use in cannon because the surface of powder in this form *increases* during combustion, whereas the surface of an unperforated cylinder grows smaller as burning progresses. This increase in area, which means an increase in rate of gas evolution, helps to sustain the pressure in the gun barrel in spite of the increase in volume behind the projectile as it moves down the bore of the gun.

The powder grains used in U. S. sixteen-inch guns are cylinders about two inches long and approximately seven-eighths of an inch in diameter, with seven uniformly-spaced perforations parallel to the cylinder axis. These perforations, which are nearly one-sixteenth of an inch in diameter, are arranged in the form of a hexagon (end view), the seventh hole being in the center of the hexagon. With a grain of this type the *web thickness*—that is, the minimum thickness of the grain between any two boundary surfaces—is less than one-fourth of an inch.

Since each multiperforated grain of smokeless powder burns in parallel layers in directions perpendicular to all ignited surfaces, and since each grain is ignited all over—inside and out—at the same time by the flash from a charge of black powder, the web thickness (about one-fifth of an inch in this case) represents the maximum thickness of smokeless powder that must be burned before the grain is almost completely consumed—not *completely* consumed, because twelve small slivers of triangular cross section remain when the web thickness has *just* burned through. However, these slivers are usually consumed before the projectile leaves the gun. This means that the total propellent charge—weighing up to 800 pounds in a sixteen-inch gun, and occupying a volume of some twenty-three cubic feet—is burned in a fraction of a second.

Although the rates of burning, mentioned above, for propellants appear rapid when compared with the rates of ordinary chemical reactions, they are actually quite slow in comparison with the velocity of detonation of high explosives, which, as indicated above, may exceed 20,000 feet a second. In fact, dynamite, TNT, or ammonium picrate would explode with such rapidity and violence as to burst the gun if an attempt were made to use one of these high explosives as a propellant. Unfortunately, premature explosions of HE shells sometimes do occur, due to some accidental cause, always with disastrous results to the gun, and sometimes to the gun crew as well. On the other hand, high explosives are just the thing for the bursting charge in bombs, shells, and torpedoes, where a quick-acting, disruptive explosive is desired.

Paradoxical as it may seem, the energy of the high explosive in a shell is actually much less than the energy of the smokeless powder used as the propellant. Again taking a sixteen-inch gun as an example, the propellant charge of approximately 800 pounds of smokeless powder has some 1,000,000,000 foot-

pounds of energy, in comparison with about 530,000,000 foot-pounds for 410 pounds of TNT, which represents the weight of an average bursting charge for the shells fired from guns of this caliber. Pound for pound, however, smokeless powder and such high explosives as TNT, Amatol, and ammonium picrate have very nearly the same energy.

Most explosives contain nitrogen and oxygen. Usually, one nitrogen atom is associated with either two or three oxygen atoms. Nitrogen and oxygen are commonly introduced into the explosive molecule by a process known as "nitration," which involves treating a suitable organic material with a mixture of nitric and sulfuric acids. Thus, nitrocellulose, the base of smokeless powder, is made by the nitration of cellulose in the form of purified cotton linters or wood pulp. Similarly, nitroglycerin, the base of dynamite, is made by nitrating glycerin, derived from fats; and TNT, by nitrating toluol, derived from either coal tar or petroleum.

In addition to nitrogen and oxygen, most explosives contain carbon and hydrogen also. This is the case with TNT, nitroglycerin, and nitrocellulose.

Now nitrogen is a peculiar, lone-wolf sort of element. Unlike carbon, hydrogen, and oxygen, it does not wish to unite with, or stay joined to, another element. It much prefers to go its way alone. So, when the proper stimulus is applied to a material such as TNT, the large, nitrogen-containing molecules fly to pieces with great violence, giving rise to smaller, simpler molecules. The nitrogen, as might be expected, goes off alone; carbon and oxygen may be united either as carbon dioxide or carbon monoxide gas. Likewise, the hydrogen may be combined with oxygen in the form of water, not liquid water, which is relatively dense, but gaseous water, which occupies a large volume. Any excess oxygen is set free as such. In the typical explosive, all products are gases, but as mentioned previously in the case of copper acetylide, the decomposition products may be solids. Black powder is another exception to the general rule, which explains why it fouls up a gun so badly. Here the solid residue is about three-fifths the weight of the original powder.

Interesting yet highly destructive explosives are fine particles of combustible materials—coal, sugar, flour, cork, wood—floating in the air. Some of the most frightful explosions on record have resulted from the ignition of such "carbonaceous" dust, particularly in coal mines and flour mills. Fortunately, such accidents are now quite rare, due largely to precautions which keep down dust.

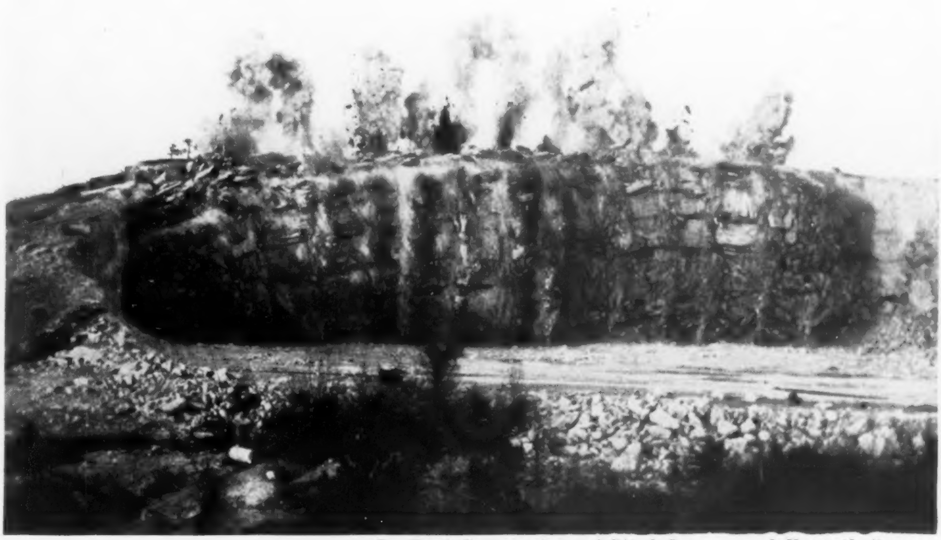
The explanation of dust explosions is simple. Each tiny combustible particle is surrounded by atmospheric oxygen with which it wishes to unite, and only the flame of a match or an electric spark may be required to set off the reaction which proceeds with explosive violence throughout the entire mass of air and dust. According to the U. S. Bureau of Mines, mixtures of coal-dust and air will not explode if the dust concentration is kept under 20,000,000 particles per cubic foot. One method for reducing dust concentrations in coal mines is to sprinkle with water containing a wetting agent, such as one of the fatty alcohol sulfates.

Metals as well as carbonaceous matter may behave in this way. Although aluminum is not commonly regarded as combustible, in the form of a fine powder suspended in the air it has been the cause of disastrous explosions. Like particles of coal or flour, tiny particles of aluminum, if heated beyond a certain temperature, combine with oxygen in the air with explosive violence. For this reason, aluminum powder is usually made by grinding the metal in an inert atmosphere such as nitrogen.

A mixture of gasoline vapor and air in the cylinder of an automobile is not

unlike a suspension of fine, combustible dust particles in the air. When ignited, each burns with great rapidity. In fact, gasoline vapor in the cylinder of an automobile may sometimes burn too fast for maximum engine efficiency. Such is the case when "knocking" occurs. Here the combustion of the gasoline vapor proceeds with explosive violence. The gasoline-air mixture actually detonates, in somewhat the same way as TNT. What is desired is not detonation, which results in energy being liberated too fast for efficient use, but a rapid, yet controlled, burning of the gas mixture. A few drops of tetraethyl lead per gallon of gasoline does the trick.

Contrary to popular opinion, the energy in a given amount of high explosive is no greater than the energy in the same amount of certain non-explosive materials. For example, a pound of TNT actually yields fewer units of energy than a pound of coal; and nitroglycerin, even if it could be "tamed" for use in running an automobile engine, would not be as good as gasoline, since, pound for pound, it cannot do as much work as gasoline. An explosive appears extremely energetic simply because all its energy is set free practically instantaneously, whereas it may take half an hour to burn a small lump of coal.



Du Pont Co., courtesy of Pit & Quarry, and Kenneth Rogers

BLASTING 69,360 TONS OF ROCK AT A SINGLE SHOT

TO DISLODGE THIS GRANITE MASS 19,266 POUNDS OF NITRAMON BLASTING AGENT WERE USED.

In this connection it is interesting to note the magnitude of the power generated by explosives in some typical applications. A sixteen-inch gun fires a projectile weighing more than a ton at a muzzle velocity in excess of half a mile a second. The kinetic energy of the projectile—approximately 265,000,000 foot-pounds—is acquired in the period of

about five-hundredths of a second during which the projectile travels from breech to muzzle. In this brief interval, the smokeless powder charge is delivering nearly 10,000,000 horsepower, which is approximately ten times the power of the combined Canadian and U. S. hydroelectric plants at Niagara Falls. A field gun of approximately six-inch caliber requires in excess of a million horsepower. These examples show why little success has been achieved by occasional attempts to obtain satisfactory ballistics with electrical guns consisting of a series of solenoids and other devices dependent upon the usual power sources.

From the preceding it is clear there are explosives, and explosives—and yet other explosives. They are "tailor-made" for a particular use. Dynamite detonates too rapidly to be used as a propellant, and is too sensitive for use as the disruptive in the big shells shot from guns. The shock of the propellant would set it off before the shell reached the end of the barrel. But TNT, Amatol, and ammonium picrate are so insensitive as to withstand such shocks, yet are powerful enough to do great damage when they do go off. On the other hand, smokeless powder does not release its energy fast enough for use as the bursting charge in shells, bombs, and torpedoes, yet is almost ideal as a propellant.

While no explosive is designed as a child's plaything, some are relatively gentle while others defy taming. If suitable precautions are taken, however, the manufacture and use of commercial explosives present no greater hazard than is encountered in many industries commonly regarded as quite safe. In fact, the personal injury frequency rate for all Du Pont explosives plants—both commercial and military, is definitely lower than that of industry as a whole. But, like "tame" lions and tigers, even the "gentlest" explosive should be handled with care by an expert to prevent accidents.

FRIEDRICH MIESCHER, 1844–1895

FOUNDER OF NUCLEAR CHEMISTRY

By Dr. JESSE P. GREENSTEIN

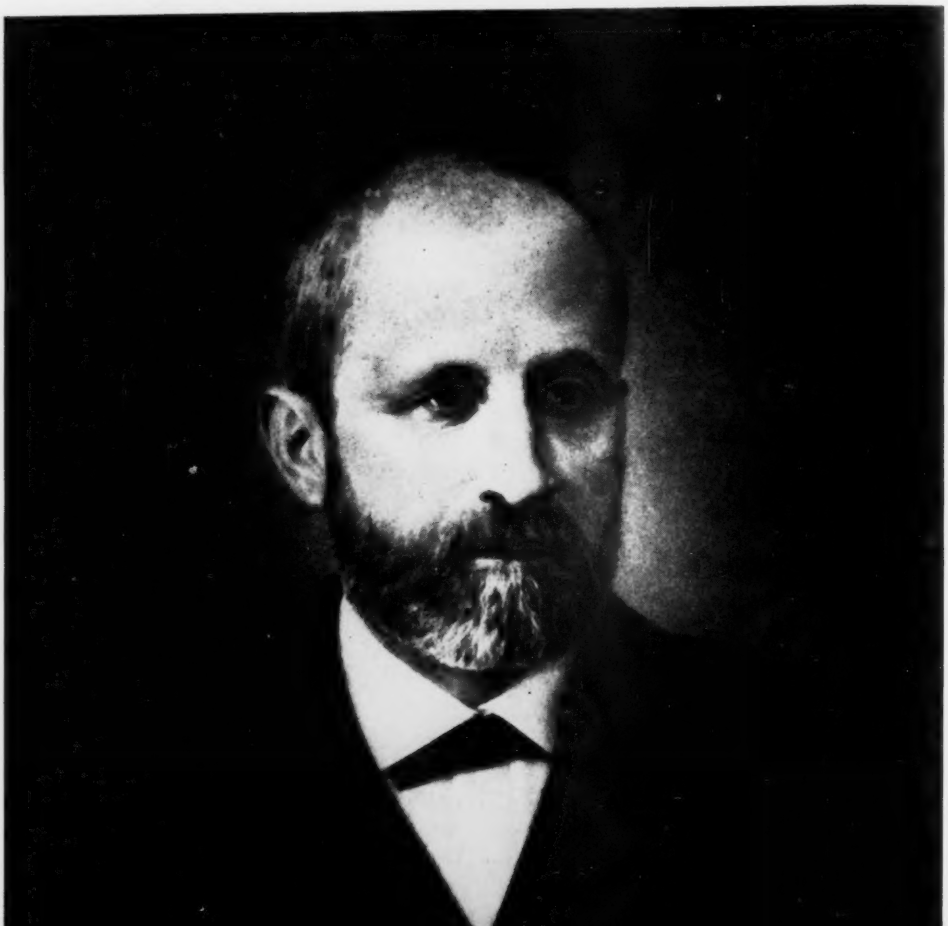
THE NATIONAL CANCER INSTITUTE, NATIONAL INSTITUTE OF HEALTH, U. S. PUBLIC HEALTH SERVICE

THE year 1944 marks the one hundredth anniversary of the birth of Friedrich Miescher, in Basel. It is appropriate at this time to recall the fundamental contributions of this distinguished pioneer in the development of biological science, and to assess these contributions in the light of the present knowledge of the subject. The stature of some men's work, if not its value, diminishes with time, and the appraisal of such work assumes largely an antiquarian interest. Chance, as a by-product of uninspired plodding, sometimes plays a major role in great discoveries. But, on the other hand, the work of some men appears to be immortal and forever new. This is because it rests upon approaches and procedures which men of science recognize as truly valid, namely: (a) the proposal of the problem in clear and unmistakable terms, (b) the conduct of the experimental solution of the problem by impeccable and sternly disciplined techniques, and (c) the interpretation of the results obtained with cool yet imaginative insight.

Miescher belonged to this latter category of workers, and in the perusal of his works the reader feels strongly their enduring quality. Briefly stated in terms of the above framework, Miescher set forward, as his essential problem; (a) the identification in chemical, as well as morphological, terms of the unique structural element of the cell, the nucleus; (b) the physical isolation and identification of the nuclear components by choosing the best available material and by employing techniques, without regard to personal comfort, which yielded the desired components in nearly unaltered form; and (c) picturing the nucleus on the basis of study of the properties of the components as a dynamic and interrelated system, the equilibrium position of which altered in response to changes, whether of intrinsic or of extrinsic origin, of any one of the components.

Miescher came of a family long distinguished in the professions. His father, one of the earliest pupils of Johannes Müller, in Berlin, was for some time professor of pathological anatomy in Bern, and later in Basel. The son studied medicine in Basel, where he had the good fortune to come under the influence of the anatomist His. First pupil, then colleague and life-long friend of His, Miescher's letters to the latter portray, even more than do his scientific papers, the thoughts which motivated his career. His early steered his young friend into the field which now is known as histochemistry, for, as the former expressed it, "in my own histological studies I came to the conclusion that the final solution of the problems of tissue development could be solved only by chemistry." It is easy to smile at His' optimism, but his statement reflects the creditable desire of the biologist to express his findings with greater precision. The replacement of biological by chemical terminology is probably not a "solution" of the problems as His envisaged it, but the latter terminology may more frequently be employed with greater clarity than the former.

Miescher obtained his doctor's degree in Basel in the spring of 1868 and in the fall of the same year enrolled at



F. Miescher

Tübingen for chemical study. Wisely, he spent the first semester there, not in research, but in sharpening his tools in the general chemistry laboratory of Strecker. After acquiring some foundation in organic chemical technique, Miescher brought the problem of isolating the nuclear components to the laboratory of Hoppe-Seyler. The attitude of Hoppe-Seyler to this problem, judged by his later actions, must have been one of benevolent skepticism, but he allowed Miescher free rein to go ahead. As source material for this investigation, Miescher chose pus cells. The choice of this material was most probably due to the very large ratio of the nuclear to the total volume of lymphoid cells. The pus cells were obtained by extraction from used bandages discarded by the adjacent university clinics, and the hardihood necessary to work with such material is probably better imagined than described; indeed, in his first published description of this work, Miescher perhaps unnecessarily explains that those bandages which stank too much were not used.

The separation of the cells from the pus fluid was not easy, for most of the extracting solutions caused clumping and slime-formation. In a letter to His, dated February, 1869, Miescher described the first successful separation of the cells by employing sodium sulfate solutions, and added the line familiar to all protein chemists, "there is no more miserable task than to attempt a sharp separation of the proteins." From the cells so obtained, the nuclei were isolated by treating the former first with dilute acid, later with pepsin-hydrochloric acid, and then with ether. On shaking, the nuclei settled to the bottom of the flask and could be filtered off. Miescher was thus the first to recognize that the nucleus possessed a higher density than the rest of the cell, a fact that has since been amply confirmed by studies with the centrifuge. The nuclear material appeared to be a compound of unusual sort to which Miescher gave the name "nuclein," and which we would now call nucleoprotein. Characteristic of the compound was its ready solubility in soda and insolubility in dilute acids, as well as its high proportion of phosphorus in firm combination.[1]

This work set the pattern for the major part of Miescher's subsequent labors, and formed the basis of his often-expressed belief that the study of the chemical phenomena of the tissues would clarify many of the processes which were hitherto obscure or inaccessible to examination by the microscope. In a letter to His, written a few years before his death, Miescher expressed the hope that by the chemical separation and analysis of pathologically-altered tissues, particularly that of the nuclear components, information on such processes as inflammation and degeneration (cancer) could be obtained, and he concluded this letter with the well-known sentence, "*Das Mikroskop lässt einen, wie man am Ei sieht, gewiss oft im Stich.*" In this sense, Miescher envisaged the field of pathological chemistry[2]

Miescher completed his work in Hoppe-Seyler's laboratory in the fall of 1869, wrote up the results of this work, and gave the manuscript to Hoppe-

[1] Up to this time, organic phosphorus, with the exception of comparatively rare instances such as Liebig's inosinic acid, was observed only in lecithin. For a long period after Miescher's discovery of nuclein, later workers referred to compounds which contained organic phosphorus as nuclein or nuclein-bodies, sometimes as nucleoproteins, without relation to Miescher's original specific designation. The curious persistence of this error only ceased when the nature of the phosphorus-containing substances became clarified, but it has necessitated great care in the reading of the scientific literature of this period.

[2] As a commentary on Miescher's vision, it may be noted that the problem *par excellence* of pathologically-altered tissues, namely that of cancer, has been approached analytically by the chemist only within the past few years, and that on the whole the independent status of the chemist in the field of pathology is at present quite uncertain and controversial.

Seyler for publication in the latter's journal, then called the *Medizinische-chemische Untersuchungen.* Miescher himself then left for Leipzig to study physiology in the Institute of Carl Ludwig, then at the height of its fame. Miescher had no particular problem in mind when he went to Leipzig and his intention was simply to learn what was going on and to master certain of the techniques. Ludwig offered him a problem on the bone marrow, but Miescher declined, believing the problem to be too complex. Also his own problems on histochemistry never left his thoughts. He was fascinated by the international character of Ludwig's laboratory and by the students who represented every famous school of physiology in Europe. Among his colleagues was the American, Bowditch, and "a Mohammedan from Cairo." In a letter to Hoppe-Seyler he states, "almost all present schools are represented by pupils or former assistants—Voit, Helmholtz, Dubois-Reymond, Kühne, Recklinghausen, Brücke, etc. Each one brings something with him, and even I, as an 'Absenker' from your laboratory, am able to give wise counsel on hemoglobin." Many conversations with the dynamic Ludwig and the beginnings of lifelong friendships with men like Boehm, Hüfner, and Schmiedeberg, made life pleasant during Miescher's stay in Leipzig.

OLD UNIVERSITY BUILDINGS AT BASEL, SWITZERLAND
RISE PROUDLY BESIDE THE RHINE. HERE FRIEDRICH MIESCHER WAS PROFESSOR OF PHYSIOLOGY.

Miescher held Ludwig's personality and teaching ability in high regard, although he pointed out Ludwig's unwillingness to take unripe men and to build them into independent scientists. Ludwig was a great virtuoso, who enjoyed the awe of a crowd of students watching him work. In a letter to his parents, Miescher relates that all the work in Ludwig's Institute is solely his, and humorously describes how each student holds a towel or a rag at some piece of work over which Ludwig presides.

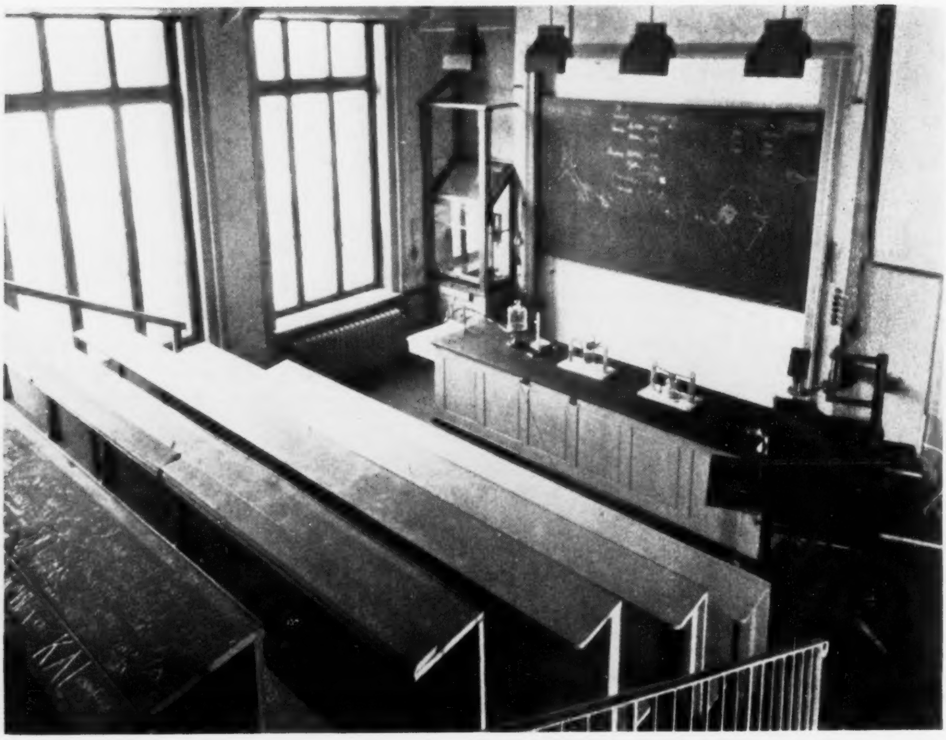
knows nothing of what is going on, and then to his astonishment finds sometime later a very fine paper in the literature under his (the student's) name alone. Ludwig followed Miescher's subsequent career with sympathy, and when Miescher lay finally on his deathbed, sent him the fine letter which included the following:

> Of course it is easier to preach patience than to practice it, and from my own experience I know what it is to give up well-loved, hopeful work. Sad as it is, there remains to you the satisfaction to have completed immortal studies of which the main point has been the knowledge of the nucleus; and so as men work on the cell in the course of the following centuries, your name will be gratefully remembered as the pioneer of this field.

In the meantime, during Miescher's stay in Leipzig and after his return to Basel in the summer of 1870, no word had come from Hoppe-Seyler as to the fate of the manuscript on nuclein. Indeed, the fate of that manuscript was destined to form one of the curious episodes in the history of science. Hoppe-Seyler was frankly sceptical of the work and hesitated to publish it. To do him justice, he probably felt keenly his dual responsibility as editor and as chief of the laboratory in which the work was done. He therefore wanted to repeat the work, and at the same time to extend such a study to the nuclei of the avian and reptilian erythrocytes. Hoppe-Seyler explained his position in a note to Miescher and suggested that, if the latter was in a hurry to publish, he send a brief communication to *Pflügers Archives.* Miescher answered that he was quite willing to wait until Hoppe-Seyler was ready to publish the work, and causally added that he noticed that Hoppe had already inserted the description of the nuclein technique in the new edition of his book. Finally, the fourth volume of the *Medizinische-chemischen Untersuchungen*, published in 1871, carried Miescher's classic, "Über die Chemische Untersuchung von Eiterzellen," and in addition a paper by Plosz, "Über das Chemische Verhalten der Kerne der Vögel-und Schlangenblutkörper."

Hoppe-Seyler's attitude to Miescher's work was best expressed in the introduction to his own paper in this volume, in which he confessed himself at first doubtful of Miescher's results but later confirmed him personally at every point. The whole affair, which was conducted on a high level of courtesy on both sides, concluded thus handsomely, and in a letter afterwards to His, Miescher expressed his appreciation of his former teacher, and concluded with the generous statement that "Hoppe-Seyler is master of the entire field which encloses histology on the one side and pure chemistry on the other." In the extension of Miescher's work, Hoppe-Seyler had isolated nucleins from yeast and from other sources. Miescher felt somewhat concerned over the superficial resemblances between these nucleins and the nuclein which he had isolated from the pus cell, and in a brief note called "Nachträgliche Bemerkungen" he warned against taking pus cell nuclein as a model for all such compounds. Hoppe-Seyler declined to publish it, on what grounds is not clear. It is true that Miescher, although later work by others substantiated his thoughts on this point, adduced no experimental evidence at the time to indicate that the nucleins from different sources were not identical.

In 1871 Miescher became *privatdozent* in physiology in the university in Basel and for his *habilitationrede* he chose to discuss the work of the Leipzig school of Ludwig, chiefly in the field of the physiology of respiration. A year later he was appointed successor to His as professor of physiology in the same institution. Previous to this appointment His had taught both anatomy and physiology and it is easy to see that he simply invested his younger colleague and former pupil with the conduct of the



PROFESSOR MIESCHER'S LECTURE ROOM IN THE "VESALIANUM."

latter subject. Miescher, although free to do his own work, was hampered by lack of space and equipment, and in one of his letters of this period expressed his yearning for the "fleshpots of the Tübingen laboratories." His work was performed in a small corner of the general chemistry laboratory, for he had none of his own; his analyses were carried out in the corridor of another building, and for assistance he had "one-fourth of an anatomy *diener* who also had to help the anatomists, the zoologists, and the pathologists." Miescher bore these difficulties not too nobly, nor very quietly, and made no effort to stifle his disappointment over the "most miserable conditions" and his misfortune to be so hampered at the "height of my powers" (he was then twenty-eight years old). Nevertheless his finest and most enduring work was performed under these conditions, as Pasteur's had been under similar circumstances in the École Normale; later, when both Miescher and Pasteur had comparatively palatial institutes built for them, their personal creative powers had waned.

When Miescher began his duties in Basel in 1871, His had been working for some time on the histology of the developing salmon egg. The fishing industry was one of Basel's busiest, and the necessary material was readily obtained. Miescher quickly observed that the salmon sperm was an excellent source of nuclear materials, and wrote enthusiastically to his Leipzig friend Boehm, "we have here within a few weeks time an enormous increase (in the sperm cell) of nuclear material which is formed within the cell at the expense of ? ?" In a later letter to Hoppe-Seyler, now at Strassburg as a by-product of the Franco-Prussian war, Miescher describes the fish spermatazoon as a model

of the nucleus of more complicated cells with an overwhelming proportion of nuclear mass. Miescher threw himself into the work on sperm with ardor and energy, and in the spring of 1872 he notified the natural history society in Basel that he had found the isolated sperm heads to be a compound of nuclein of high molecular weight (now called nucleic acid) and a base standing between urea and proteins in complexity, which he called "protamine." The sperm head was largely a salt of nucleic acid and protamine which did not show the properties of a simple mixture of the two substances.

This paper of Miescher's, published in the sixth volume of the *Verhandlungen der naturforschenden Gesellschaft in Basel*, is a landmark in the history of biological science. In the beginning of the paper Miescher pointed out that the testicles of salmon in March weigh on an average fifteen to twenty grams, whereas in November of the same year (the time of spawning) the average weight is three hundred to four hundred grams, and sometimes more. He briefly speculated as to the source of this increase in weight. The sperm head could be dissolved in strong salt solutions from which fibers precipitated on dilution with water.[3] The nucleic acid could be separated from the protamine base by extraction of the defatted sperm with weak acid, whereby the base was removed and the acid left as the residue. The former was purified as the platinum salt; the latter was purified by dissolving in weak alkali followed by precipitation with alcoholic acid.

[3] Fibrous precipitates obtained in this way from tissues were long studied by biologists, who generally mistakenly confused them with the myosin fibers of muscle. In recent times, Bensley, and Mirsky and Pollister, reviving this method, observed that the material so obtained was largely composed of nucleic acid.

THE "VESALIANUM," INSTITUTE OF ANATOMY AND PHYSIOLOGY, BASEL.

Miescher then went on to describe the sperm head as, "an insoluble, saltlike compound of a highly nitrogenous organic base with a phosphorus-rich, acidic nuclein (nucleic acid)." That nucleic acid was somewhat in excess within the sperm he could prove by adding free protamine to the latter and obtaining a new precipitate of an acid-protamine complex.

Miescher's insight into the properties of these components was profound; he did not view the sperm complex as a static but rather as a highly fluid system, the composition and properties of which varied markedly according to the conditions present. Toward the end of this classic paper he pointed out that sodium chloride, protamine, and nucleic acid formed a three-component, dynamic system, the equilibrium point of which was governed by the relative concentrations of each of the components as well as by the *Alkalescenz* (pH in modern terms).[4] The basic reason for these dynamic interchanges Miescher recognized in the polyvalent character of both nucleic acid and protamine and in the ionic dissociation of the salt.

The idea of a state of flux, of a dynamic interchange among the organic and inorganic components of the cell, was always strongly emphasized by Miescher. In a letter to His, dated May 1876, he discussed the equilibrium forces among the protoplasmic components, and included the following observations which are worth quoting:

> The thought always occurs to me that the proteins are really both strong acids and strong bases, which possess a neutral reaction only because of an inner neutralization. If one mixes sodium chloride with protein there must occur protein chloride, sodium proteinate, and protein-proteinate. Different proteins have different affinities, and even the insoluble proteins are not unreactive.

Thus was stated the amphoteric character of the proteins long before it was established by the work of Küster, Bjerrum, and the workers of the present day.

Ruefully, Miescher added that while he intuitively recognized the presence of equilibrium forces he lacked the ability to formulate them—"lacking in the knowledge (of these forces) which is of the magnitude of the Gotthardt Tunnel, I can only find myself a mouse-hole." Nevertheless, nowhere has biochemical dynamism in the form of protein reactivity been better expressed.

The work on the ripe salmon sperm was subsequently extended to include the sperm of the frog, bull, and carp. Although nucleic acid could be readily isolated from the latter sources, no protamine was evident. Protamine was likewise absent from the unripe salmon testicles. Combined with the nucleic acid in these materials was, instead, a sulfur-containing protein of considerably greater complexity than protamine. Miescher was puzzled over these findings, and in a letter to Boehm dated January, 1873, declared that the presence of protamine in the salmon sperm was a "miserable, special case," and suggested that the "pompous designation (of protamine) be immediately buried." The unique character of the protamines and their ontogenesis was recognized later by Kossel, partly on the basis of Miescher's work on the development of the salmon gonads and partly from his own fine analytical data on the constituents of the protamines.

But most of Miescher's attention was lavished on the preparation of nucleic acid from various sources. Recognizing the lability of this material, he spared no effort to obtain preparations in as unaltered a form as possible. In a posthumous paper edited by Schmiedeberg he states that all work was performed in a room at two to three degrees centigrade or less. His working day, he described in a letter to Boehm: "When nucleic acid is to be prepared I go at five o'clock in the morning to the labora-

[4] Confirmed in all details subsequently by Hammarsten and his colleagues in Stockholm.

tory and work in an unheated room. No solution can stand for more than five minutes, no precipitate more than one hour before being placed under absolute alcohol. Often it goes on until late in the night. Only in this way do I finally get products of constant phosphorus proportion." Miescher's analyses of his preparations, as Levene and Bass have pointed out, compare favorably with the best of the modern analyses of nucleic acid. Yet Miescher, by temperament, was not an analyst. Over and over again he rather mournfully described the effort devoted to his analyses as *Fabrikarbeit* and as an activity which "prospers best when least accompanied by imagination." Nevertheless he appreciated the value of such effort and in a letter to Boehm remarked that "always I ask myself if histochemistry could only be conducted otherwise (without analysis), and always I return to my phosphorus, fat, and other determinations, as a necessary control and assurance against disappointments with the work with the microscope." Working under such stringent conditions with so little assistance, his frequently expressed impatience is easy to understand—"if one could only live for a couple of hundred years and not have a pack of hounds on his tail, it would be a pleasure to work on these problems, but *vita brevis, ars longa. . . .*"

Possibly, in part as a relief from the exacting work on the isolation of the nucleic acids, and in part as a result of the stimulation of His, Miescher undertook, about 1875, the problem of studying the transformation of the salmon tissues during the period of gonadal development. It was long known to fishermen that the salmon did not eat as long as they were in fresh water. As a well-nourished animal with undeveloped gonads, the salmon leaves the sea, enters the Rhine at Holland, and, swimming upstream, remains for many months during the summer and early fall in the upper, sweet-water reaches of the river. During this period the sexual organs mature at the expense of the rest of the fish's body. After spawning, the hungry fish rush back to the sea. During the stay of the fish in sweet water there is evidently a chemical breakdown of the tissues, a transfer of the split products to the site of the gonads, and a synthesis at the latter site. With great energy, Miescher studied thousands of salmon (readily available in the great Rhine-port of Basel), weighed the muscles, livers, spleens, blood, and gonads, and studied the last mentioned both histologically and chemically throughout the developmental stages. Only part of this gigantic work was published during Miescher's lifetime; the remainder was collected and edited later by his friend Schmiedeberg.[5] In the work published in 1880, Miescher described the balance of protein, fat and phosphorus in the sexually-developing salmon, and showed that it is the musculature of the fish which supplies the material subsequently synthesized in the gonads. Those muscles most affected possess, during this season, the poorest blood supply, and Miescher expressed the belief that the "liquidation" of the musculature (lysis) was facilitated by comparatively anaerobic conditions. For synthesis of tissue proteins, on the other hand, a rich source of oxygen was necessary. The concept that the equilibrium between lytic and synthetic processes was regulated fundamentally by the tension of the available oxygen was frequently emphasized by Miescher throughout this work, and suggested a fruitful approach to many problems in physiology.

Engrossed as Miescher was at this period (1876) in the work on the salmon and in the conscientious conduct of his teaching duties, he received to his dismay an order from the Swiss govern-

[5] *Statistische und biologische Beiträge zur Kenntniss vom Leben des Rheinlachses in Süsswasser.*

ment to investigate and prepare a report on the nutrition of the inmates of the Swiss prisons. Miescher frequently called this task the most troublesome and thankless of his life. He realized that little could be done in view of the inadequate knowledge of nutrition at the time, and that which he tried to do only embroiled him in profitless controversy both with the authorities and with the commercial companies. He bitterly attacked the diversion of fresh milk from the needs of growing children and nursing mothers to the manufacture of cheese for export, and despite his official position did not hesitate to write indignant letters to the daily press in support of his views. Nevertheless, to his private consternation, he was recognized as the Swiss authority on nutrition, and wryly described to His his status as "the watchdog over the stomachs of three million fellow countrymen." Perhaps Miescher's most important contribution in this field was his recognition of the indispensable role of the proteins, particularly the animal proteins, in human nutrition.

At the close of his great work on the salmon, about 1882, the government constructed for him a fine research institute in Basel which was opened with much ceremony in 1885 and later nicknamed the "Vesalianum." But the creative period of his life was over, and he suffered from fits of lassitude and depression, forerunners of the tubercular disease to which he was to succumb. The long days and nights spent over many years in rooms around zero degrees Centigrade had begun to exact their toll. Depressed by the waning of his strength he wrote with wry humor to Boehm, "the basic feeling of my life is that uncomfortable sensation of a man who has lost the buttons of his suspenders" and, "every night I go to bed with the feeling of a schoolboy who has not learned his lesson for the day." "I know better than anyone else," he wrote to His, "that my work is only the preliminary study to a future Histochemistry." All this came hard to Miescher, for his was the intense belief that creation was the first law of the spirit. "One should always do something by oneself," he wrote to an old friend, "no matter how small." During the last few years of his life, he devoted his remaining strength to the stimulation of the interest of younger men in the field of physiology. "A professor without young collaborators," he wrote, "is *'nur so ein verstümmelte Weidenstrunk.'*" His teaching was cast in a stern mold, and appealed only to his better students. Jaquet, one of his assistants, quotes Miescher as saying, "hard work develops the investigator's talents, and he who has not learned to surmount difficulties in his youth cannot later cope with them." The research work in the institute during this period was carried on almost exclusively by Miescher's pupils and was concerned with the physiology of respiration. This study took two forms, the development of new apparatus for measuring blood gases, and field work on the blood components at high altitudes. The latter approach revealed that the blood count of individuals rose as they ascended into higher altitudes and decreased as they descended.

Miescher died in 1895. His life's credo is best expressed in a letter written to Boehm in 1871: "I believe that in the organic world each complicated case is built on simpler things, and these in turn on still simpler things . . . each case must be reduced to its simplest terms." This belief, carried into practice, was the basis of the founding of a new branch of science.

ANCESTORLESS MAN: THE ANTHROPOLOGICAL DILEMMA

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CONFRONTING me is one of the new ponderous volumes which it is now the fashion for sociologists to issue in the field of general sociology. As a teacher of anthropology and sociology, I am critical of the genealogical trees built by scientists purporting to show the evolution of man from pre-human ancestors through successively ascending human levels.

The tree in this book formally repeats the patterns of genealogical trees which were begun last century. The present book does not prove nor even pertinently illustrate the fact of evolution. It is all branches and terminal twigs which have no fibers running through the trunk specifically connecting with definite ancestral forms. So men, those of the present and of lower levels, stand ancestorless. The accompanying interpretative explanations are quite as vacuous. This is a fair sample of the genealogical trees now appearing in sociological, anthropological and biological works.

It is evident that anthropologists assume and profess to be evolutionists. They probably would resent the suggestion that they are dogmatists in the biological field, yet such appears to be the case regarding the subject under discussion. By their works ye shall know them, and it is by their work products in this particular field of evolution that they are now to be judged. Their genealogical trees purporting to show the evolution of man should satisfy theological fundamentalists who reject the idea of such evolution. In fact they are empty forms which consist of nothing but assumed roots, trunk, many limbs which grow in number through the years, and human twigs terminating the trunk which are supposed to connect with the assumed roots. Now a tree that is constituted wholly of limbs does not tell us much. Limbs below do not beget the limbs above them. They are not ancestral to them, only cousins to what is above them. The anthropoid limbs on these genealogical trees above the roots are the various living apes: gibbon, orang-utan, gorilla, chimpanzee; and the human levels: Pithecanthropus, Sinanthropus, Heidelberg, Eoanthropus, and Neanderthal. Present races are the twigs at the top. The trunk is empty of content from twigs to roots, which represent an extinct ape. Living apes and the various levels of men between apes and present men are asserted to be too different from recent man to be his ancestor. So present man's lineal ancestor is supposed to be some Miocene pithecan who lived some fifteen million years ago and which must have differed from the recent humans tremendously more than do the intervening forms. The present writer is an evolutionist and believes he may perform a needed scientific service by calling attention to some of the inconsistencies in the situation.

Anthropologists share genealogical conservatism along with biologists generally. Biologists are, of course, confessedly evolutionists, but it is really remarkable how little evidence they admit in support of their position. This is so true that a scientific writer published an article a few years ago in which he took the position that since evidence linking modern species with the ultimate primal form is lacking, it is necessary to think that they have been ex-

tant since the earliest phyla appeared. Further, since there are no evidenced connecting links between the linear series of forms developing from those original phyla we must think of evolution as only meaning the development from level to level of forms which took place within those linear series. Finally, since even within those linear series, as is found in the case of dog and anthropoids, there is an absence of small gradual variations leading from variety to variety, it is necessary to think the gaps were bridged by great mutations.

The conventional genealogical conception originated out of the discussions of Darwin, Wallace, Haeckel, Vogt, and others in the first part of the last half of the last century. It was elaborated and more fairly established by Broca, Kollman, De Quaterfages and others of the eighties and nineties of that century. As we read about how they reached their conclusions, we are impressed by the fact that bold guesses and generalizations were made which were supported by a paucity of water-tight proof. From time to time as new finds of pithecans and humans have been made, their types have been added as limbs on the genealogical tree at the supposed appropriate point. This pattern had become traditional by the beginning of this century. Now when genealogical trees are constructed they are apt to follow this traditional copy. We will note how these gaps are treated by some recent anthropological authorities.

A discussion of the origin and evolution of man must center in the concept, man, as distinguished from concepts of other forms of life. There are only a few certain criteria of man. Where these are present in an organism it must be pronounced human. Where they are absent, the organism must be regarded as non-human. These criteria are: rectilinear erectness, non-opposable great toe (a walking, not climbing, toe), flat footedness, general hairlessness of skin covering, comparatively great brain capacity. All humans possess all of these traits, while they are absent in all simians.

The evolution of man embraces three large general levels of organic forms, and of course many smaller ones. It will prove illuminating to examine the three large gradations for the purpose of observing the hiatus between them left by professional and authoritative anthropological writers. These gaps exist between man's sub-human ancestor and primal man, between primal man and Neanderthal man, and between Neanderthal man and present man, ***Homo recens***, or *sapiens*.

There are three forms of living great apes which stand closest to man in anatomical respects: gorilla, chimpanzee, orang-utan. They are humanoid in most essential traits. They deviate from recent humans in size of brain, in being quadrumana for locomoting purposes, in being hairy, in being heavily prognathic, in having beetling brow ridges, and in having a climbing foot with opposable big toe rather than a walking foot. Their average brain capacities range from some 300 to some 550 cubic centimeters. Probably this is the most important difference from man as it limits simian level of intelligence to one of childish humans. The recorded cranial capacity of the modern European type of man ranges from 880 to over 1800 cubic centimeters. The average may be set at 1550. If we regard the average brain capacity of great apes as 400 and give that a value of 100, then the average brain capacity of modern European types possesses a value of 386, almost four times as great. It would require an unthinkable mutation to step up to the higher form of anthropoid life.

There is large agreement among recent anthropologists to the effect that man's direct ancestor was an extinct

ape. It is apparently the method of evolution they espouse (more or less conventionally) which leads them to hold that living apes differentiated too recently to give time for the evolution of humans from them. Anatomically, apes are very similar to humans. Professor Hooton in his *Up From the Ape* abundantly shows this. They are thousands of times more similar than dissimilar. They are so much like us that we have scientific difficulty in assigning them anything like a remote classification. The general fashion has been to erect living apes into one family and human beings into another. But Professor H. H. Wilder says that, anatomically, this is not permissible. Since they are too much like humans, he includes all living apes and man in one family, *hominidae,* and places under this three sub-families: gibbon, the great apes (gorilla, chimpanzee, orang-utan), and man. In general it may be said that living apes are so much like the fossil apes that lived in Miocene times some fifteen or twenty million years ago, and which are supposed to be ancestral to both living apes and man, that what is said of the former may also be attributed to the latter.

There is an anthropological dogma that some type of Dryopithecus, which lived far down in Miocene times, is the most promising candidate for the position of man's simian ancestor. This type of ape was widely distributed in Asia, Europe and Africa. Recently it has been found to have possessed two distinctively human skeletal traits which other ancient simians did not have. One was the *linea aspera* of the femur bone which experts say is a certain indication that the body occupied an erect position; for it is a vertical projection on the posterior side of the femur to which the extensor muscle from hips and back attaches itself, thus making possible the erect attitude. Only erect animals have this appendage on the femur. The other characteristic was pentacuspid molar teeth. In his *Origin and Evolution of Human Dentition,* Professor W. K. Gregory demonstrates our dental lineage. More than almost any other organ, teeth hold their traits through millions of years. Man's teeth, along with those of other primates, are pentacuspid and are thus differentiated from those of other mammals. So here was Dryopithecus, an ancient Miocene simian with pentacuspid teeth, a definite primate mark. Thus Dryopithecus qualifies in two definite respects to serve as man's remote pithecan ancestor. But anthropologists and biologists furnish us with no series of specific connecting links with that ancient pithecan, and they reject as spurious the only series we do possess. These are now to be discussed.

The earliest recorded representatives of the human level are Java Man (*Pithecanthropus erectus*) and Pekin Man (*Sinanthropus pekinensis*). The first skeletal remains of the former were unearthed in central Java by DuBois in the seventies of last century and those of the latter in limestone caves near Pekin since 1927 by Davidson Black and his scientific associates. The former had a brain capacity of about 900 cubic centimeters, that of the latter being slightly greater. The straight, slender femur of the former with its *linea aspera* calls for a walking, upright individual, the few teeth found are humanoid, and the brain capacity is a half larger than that of the largest pithecan skull ever measured. Skulls, teeth, and bones of wrists and knuckles of the latter denote an erect human being. Some authorities rate them as of different species or types; others think they are specimens of the same species. Since their differences are much less than those which obtain among modern men, even among those of the so-called "white race," it seems superficial to try to regard them as two species or varieties.

Before discussing the relation of this human type to apes and to other human levels, we should allude to several representatives of what is sometimes called the Ape Man of South Africa. The brain capacity is that of the largest gorilla brain, the teeth are human-like, and leg bones and head balance indicate an organism that walked upright. It may be a link between pithecoids and humans.

Genealogical tree builders show what they think of the relationship between *Homo recens* (modern man) and Java-Pekin Man by the position they give the latter on that tree. He is placed far down the trunk as a very lowly limb, thus indicating he is not in our direct ancestry but is a distantly related cousin or uncle. We will allow two current representative anthropologists explain why this position is assigned. Here is Dr. J. H. McGregor speaking:

> At the present day, in the light of later discoveries of early Pleistocene man, no one believes that Pithecanthropus is a "missing link" in quite the same sense that Dr. DuBois first considered it, but still in a way it is a "link" in that it is more apelike as regards the skull and brain than any other human family, it is certainly the lowest in brain development and, by inference, in mentality and cultural capacity. . . . Pithecanthropus may not be directly ancestral to any other human type, but it may mark instead the end of its particular branch of the family. Even in this case it would be vastly important as representing at least a collateral ancestor—a great uncle rather than a grandfather.

The position of Professor Hooton is very similar.

> It really matters very little whether we consider Pithecanthropus a giant ape belonging to an extinct branch nearer to the human line than any other anthropoid stock, or whether we decide that the Java primate is an early and obsolete humanoid form which diverged from the main stem of development before the evolution of essentially human types. It is of small moment whether we are to call him "Grandfather" or "Grand Uncle." It seems improbable that the 500,000 (many geologists say 1,000,000) years which have elapsed since Pithecanthropus was smothered in ashes, have witnessed the evolution of his progeny into modern man. Such time is too short for so rapid a development.

So it appears to be "scientific" to guess him off as a trial evolutionary balloon which began back in Pliocene times and finished its course in late Pliocene or early Pleistocene.

This Java-Pekin kind of human is thus put out of the running for being our ancestor because to it is imputed considerable or great anatomical differences. The chief differences they see are smallness of brain, heavy torus (eyebrow ridge), low retreating forehead, and more or less pronounced prognathism. The most telling differential is doubtless that of brain size. Now, I am not seeking to prove that Java-Pekin is our immediate ancestor but only attempting to make an approach for a fair scientific appraisal. In order to accomplish this I will indicate the range of differences in cranial capacity obtaining within the modern race itself and show that it does not exceed that existing between the latter and Java-Pekin Man.

The smallest cranial capacity of an adult modern is that of a Tyrolese woman that registered only 800 cubic centimeters. Comparable with that was the capacity of the Australian female of 930 and of the Dravidian Bheel of 940 cubic centimeters. The greatest capacity stands at 1800 cubic centimeters or more, let us say 1800 for convenience. Now if we give the Tyrolese brain capacity a value of 100, that of average European brain capacity becomes 204. The latter capacity is more than twice that of the former. In like manner let us assign a value of 100 to the average cranial capacity of Java-Pekin Man, 950 cubic centimeters. Then the least modern cranial capacity, 880, possesses a comparative value of 92.5, very much smaller rather than tremendously larger. Then, if we compare the brain capacity of Java-Pekin with the average brain

capacity of modern Europeans, 1550, we secure this startling result. Giving the former a value of 100, the latter has that of only 163. This is a far smaller range of cranial capacity than occurs within *Homo recens*. The average European brain is only 63 per cent. greater than that of Java-Pekin, while the greatest brain capacity of modern man is 102 per cent. greater than that of the smallest capacity within that group. Anthropologists place Java-Pekin low down in the genealogical tree as a mere limb. In terms of brain capacity where would an authoritative anthropologist place the Tyrolese woman or the Dravidian Bheel? They assign them a position among the tipmost twigs of the genealogical tree because they happen to belong among moderns. Judged by brain capacity alone they could not qualify for a place among our ancestors on the genealogical tree. They could only be low down limbs on that tree.

Likewise, it could be shown that in respect to other physical traits modern man exhibits as great intra-human differences as those between Java-Pekin Man and apes, between Java-Pekin and Neanderthal Man, and between the latter and present man, ***Homo recens***.

Anthropologists usually dispose of Neanderthal Man as another evolutionary balloon which nature sent up in its effort to evolve full-fledged human beings. The Neanderthalers lived in Europe during the last third of the ice age. This developmental strain, some suggest, possibly may have passed through the Heidelberg level in its upward course, as jaws and teeth of Neanderthal and Heidelberg somewhat resemble. But this attempt to realize full-orbed man ended abortively, according to conservative and traditional anthropologists, never having arrived at first base as Cro-Magnon Man. So this second great level of man is not honored as being ancestral to modern man, but finds its place as uncle or cousin among other branches of the genealogical tree. It was suggested previously that a branch is not ancestral to other branches above or to the tipmost twigs of the tree.

The first specimen of Neanderthal Man was found in a limestone cave in the Neander river valley of Germany near Dusseldorf in 1885, hence the name, Neanderthal. Since then the remains of some twenty Neanderthalic individuals have been unearthed. The greatest density of distribution is found in France and Belgium. From there the finds spread eastward through Europe and southwestern Asia, and possibly as far eastward as China. Hrdlicka would class Pekin Man among Neanderthaloids. It is patent that this kind of human is well authenticated. Liberal estimates set the beginning of this type in Europe at about 150,000 years ago during early or middle third interglacial. Conservative estimates, based on stratigraphical rather than radioactive methods of computing geological time, set the further time limit at 75,000 years ago. The last of the more extreme Neanderthalers ceased to exist (so far as finds go) about 25,000 years ago, during the latter part of the last great ice sheet. The time span of the pronounced specimens of this people in Europe was thus anywhere from 50,000 to 125,000 years.

Anthropologists usually describe this type of man according to the physical traits of the more extreme specimens rather than by those of average individuals, the proper scientific mode of identifying species and varieties. So described, the more outstanding Neanderthal traits are: Large torus (eyebrow ridge), low retreating forehead, relative chinlessness, prognathism, low stature, massive chest and shoulders, forward flex of knee and stoop of head and shoulders (greatly exaggerated), and undeveloped heel bone (calcaneum). In average Neanderthalers these traits are

reduced and in the variants toward the other extreme (those with less and less) they all but disappear. So there exists a range of variation in these traits from glaringly obtrusive on the one side to feeble manifestation on the other.

There is need for emphasizing the fact that while one or more Neanderthalers glaringly manifested several of these characteristics, probably no one individual had them all in a pronounced way and certainly not all the traits pertain to all individuals of that class. The score of individuals which make up this group were more Neanderthal generally than are members of any present racial stock, but to the latter belong many individuals which in one way or another are more Neanderthal than were the Neanderthals generally or than most Neanderthals particularly.

The difference between cranial capacity of Neanderthal and *Homo recens* is much less than the range of difference exitsing within either group. We have seen that the mean cranial capacity of Europeans is 1550. That of four Neanderthalers (Neanderthal, male, 1400; La Chapelle aux-Saintes, male, 1600; Gibraltar, female, 1300; LaQunia, female, 1370) is 1418. If we allow a value of 100 to the Gibraltar skull, La Chapelle has a value of 122, only 22 per cent. greater. This is relatively insignificant as compared with the 102 per cent. difference existing between the smallest and approximately largest modern brain. It is also much less than that found between smallest (1100 cc.) and largest (1600 cc.) Neanderthal cranial capacity. The latter has a value of 145 in comparison with the former, nearly a half greater. The average European brain is only 14 per cent. larger than the average Neanderthal brain. The intra-Neanderthal differential cranial capacity is three times and the intra-*Homo recens* differential capacity is over seven times greater than is the difference between the average cranial capacity of Neanderthal and *Homo recens*. Scientists have no justification on such grounds for rejecting Neanderthal as ancestral to modern man.

Conservative anthropologists generally reject Neanderthal Man as the forebear of Cro-Magnon and recent man but they do not tell us who our ancestor was. The traditional position is that Neanderthal Man entered Europe catastrophically and disappeared catastrophically. He pushed into that continent suddenly, it is held, since there was no anatomical premonition in other human types, and since his flaked stone implements were great mutations and not introduced by evolutionary stages from preceding chipped stone implements of Europe. Then it is maintained that his type of anatomical form disappeared suddenly without symptoms of evolution into later forms and that his culture, flaked stone implements, as suddenly gave way to the Aurignacian chipped implements. But Dr. Hrdlicka takes an evolutionary point of view of the appearance and disappearance of Neanderthal. He develops his ideas in a telling paper in the annual Smithsonian Report of 1928. His strongest evidence comes from the side of culture rather than from anatomy. He shows that there was a gradual evolution of stone implements from the preceding Acheulian chipped kind to the flaked implements of Neanderthal. He cites expert archeologists who work European stations to the effect that no one can make a definite line of demarcation between pre-Neanderthal and Neanderthal implements, nor between the latter and Aurignacian artifacts. His study of the foods of pre-Neanderthal and Neanderthal people indicate that there was no great divergence in kinds and forms of food. A compilation of data on dwellings shows that here had been a gradual trend away from open living sites to stone caverns before Neanderthal, that Neanderthal people continued the devel-

opment trend toward caves, and that early Aurignacian (Cro-Magnon) men continued the line of development. Dr. Hrdlicka visited all the museums of Europe and measured their Neanderthal skeletal remains. He concluded that, while there is a type of skeleton which may be recognized as Neanderthal, there is wide variation in the type which manifests itself in nearly every skeletal organ. He thinks that this variability foreshadows an evolution into a later form. Consequently Hrdlicka is an evolutionist regarding the origin and the "disappearance" of Neanderthal man. He maintains that Neanderthal man evolved into Cro-Magnon man and that the latter evolved from and out of the former. So we have at least one authoritative physical anthropologist who traces the descent of recent man from Neanderthal.

It is interesting to compare the position of such an ardent evolutionist as the author of *Up From the Ape* with that of Hrdlicka. Professor Hooton, in one passage, is certain that Neanderthal man utterly and suddenly disappeared from Europe. This event took place "sometime after the maximum of the last glacial advance." Ignoring the work of Hrdlicka, he asserts that his culture in the caves of Europe was replaced by one that was "quite different," that of the Aurignacian people. It is "inconceivable that they [the latter] are the descendents of Neanderthal man since there is little or no transition between the types." So Neanderthal man is relegated to the place of limb on our genealogical tree.

Dr. Hrdlicka recognizes that the Neanderthal type of man often occurs among European peoples in Europe and elsewhere. To him this is an evidence of the descent of present man, including Cro-Magnon, from Neanderthal. Professor Hooton recognizes this fact, too, but thinks it may be explained in some one of three ways: (1) Chance variation. But the cases are too numerous to be so explained. (2) "Evolution of modern man from a Neanderthaloid ancestor." But such an ancestor would differ from *Homo recens* more than does Neanderthal. It is easier to think of our evolution from Neanderthal than from a more primal and divergent type. (3) Such reappearances may be the "result of a hybridization of Neanderthaloids with some modern form of man." Professor Hooton favors this choice which might seem to recognize continuation as an evolutionary factor. One might expect that if there were types of modern men coexistent and in contact with Neanderthaloids, miscegenation would occur and that various kinds of progeny would ensue, some of which most certainly would be Neanderthaloid. The known record of racial evolution is characterized by intermixture and miscegenation of racial strains, so much so that there are no "pure races," polyracism is universal, and the recurrence of former racial types is likely everywhere. Such mixture is part of "racial evolution," which is a way of saying that it is a mode of the evolution of Neanderthal man into present-day man. So Professor Hooton unwittingly really seems to make a place for the evolution of Neanderthal into modern man. It is interesting to note that current anthropologists are curiously reticent about mentioning Dr. Hrdlicka's 1928 paper on Neanderthal.

Professor Hooton's contention that Neanderthal man could not evolve into Cro-Magnon man because the latter is a definitely different and advanced type loses some of its strength in view of what he says about Cro-Magnon. He contends that Cro-Magnon people were too variable to constitute a "racial type." He says that that so-called race does not possess "a single feature" which does not have "wide inter-racial distribution." The "so-called Cro-Magnon race

is nothing other than a hybrid type resulting" from an admixture of dolichocephalic (lond-headed) and brachycephalic (broad-headed) peoples. If Professor Hooton is correct, we have to conclude that Neanderthal man had nothing definite in the way of a Cro-Magnon race to evolve into. And Dr. Hrdlicka tells us that Neanderthal was a greatly variable type. Hence, we conclude that since the first *Homo recens* was so variable and since Neanderthal man was so variable, since there was no consistent racial type in either case, the idea of evolution of the one into the other does not seem impossible.

In view of the position of anthropologists that Neanderthal, Eoanthropus, Heidelbergensis, and Pithecanthropus can not be in our ancestral line because of considerable differences in anatomical structure, the question arises as to what gradation of difference they *would* accept. How could any essential structural difference whatever be accepted? Just what angle of deviation from spinal erectness will be admissible as being within the range of human kinship? What maximum degree of differential of knee tip, of pelvic deviation, of astrigulus divergence, of cranial capacity, of prognathism will be acceptable? We should be informed regarding the admissible degree of organic variation, but that matter is never explicitly discussed.

The conception of the evolutionary origin of man should be formulated in light of the great variability obtaining among men today. Racially humans are the so-called "white," "black," and "yellow" races, three or four great general racial types, which in turn differentiate into multitudinous racial and subracial kinds. Within each race and subrace exist very numerous and wide variations. The estimated number of races varies from three or four to thirty or forty, depending upon the criteria set up by classifiers. As an example, one of our leading anthropologists today, Professor Hooton, breaks present mankind down into thirteen races and fifteen sub-races, a total of twenty-eight. The mongoloid division is the only one he does not divide into races and sub-races. Deniker (1913) established twenty-nine racial types.

In order to glimpse this variability that exists, let us inspect one of the "great divisions of mankind, the so-called white race," in respect to a few of the physical traits which are regarded as criteria of race. It is called the "white race," but note its color variations. Its color graduates all the way from the lightest white, as seen in some Nordics, to olive and brown, as manifested among Berbers and Arabs. About the best that can be said for the white race being white is that its color average is somewhat lighter than that of other races. Some individuals of certain other racial groups are whiter than some individuals of the so-called "white race."

The form of head is so divergent among "whites" that we cannot recognize it as a criterion at all of membership in the white race. The range of cephalic index (the per cent. horizontal head width is of horizontal head length) is from extreme dolichocephalism (long-headedness) to extreme brachycephalism (broad-headedness). We are not able to say definitely that the white race is more dolichocephalic than other races. And what is said about head form is about as true of stature. Probably if we had the average height of all "whites," "blacks" and "yellows," the white average would be a little greater than that of either of the other groups. Nevertheless some of the tallest men on earth are found among the Soudan Negroes. The Akka Negrilloes are the shortest people with an average height of 54 inches, but Scandinavian Lapps are short whites, having an average stature of only 60 inches. The range of color of eyes

among whites runs from very light blue to lustrous black. The hair of whites varies in linear form from almost straight and lank to very curly, almost frizzy, so that the white race shares the forms of its hair with other races. Only one race, the Negroid, has a monopoly on any one form of hair, kinky hair. Kinkiness is about the only certain criterion of race, and kinky hair is the only kind of hair "pure" whites do not have. Caucasoid noses vary from low and broad to high and narrow. On the average, the white nose is a little higher and thinner than that of any other racial division, but it has no monopoly on most forms of the nasal appendage. In contour of face, no race has a sole claim on orthognathism (straight alignment from forehead to chin) or prognathism (projection forward of jaws, especially at level of teeth). All we can truthfully assert is that probably the average of white faces is somewhat more orthognathic than are those of non-white groups. Nor is there anything peculiar to whites in bodily posture, members of other races being as rectilinearly upright as they.

What obtains among whites regarding variability in physical traits also is found among other types of mankind. The variability is so great that as a consequence it is next to impossible to classify mankind into hard and fast racial types. The distance in range of variability between the extremes respecting these racial traits within *Homo recens* in most cases is greater than that obtaining between the averages of the various evolutionary levels of mankind.

Some of the anthropological rulings against kinship descent and for establishment of different types because of seemingly not very great skeletal differences are patently untenable. As a matter of fact, we are all the time surrounded by persons whom we assign to one or another race amongst which there are far greater differences in structural build than there are between Java Man and Pekin Man, or between Neanderthal and modern man. The writer has been measuring students for many years and has collected data on head and stature measurements, color of skin, hair, and eyes, gnathic angle, form of hair, etc., of hundreds of individuals. His residential state has one of the largest Scandinavian populations proportionally of any state. Nearly half the students are of that stock. Yet within this group are found startling differences. Skin, hair, and eye color range from lightest light to almost darkest dark. Hair form varies from almost straight and lank to exceedingly curly, almost frizzy. Stature runs from short to very tall and build from stocky to slim. The fluctuations of the cephalic index are extreme, ranging from 70 or less up to 100. Brain capacity varies from an approximate 1200 to 1700 cubic centimeters and over. Within the compass of the same cephalic index, crania present violently different contours, some full where others are scant, high, medium, and low. During 1941–42, among those studied were two brothers. In complexion, color of eyes, and in color and form of hair, they were fairly similar, but in cephalic index they stood far apart; that of one being 79 and that of the other 100. In stature, one was small and slight and the other was tall and robust. Some of the differences between Java Man and Pekin Man almost pale into insignificance in comparison with those which obtain within this Scandinavian stock. Could the skeletal remains of these students be placed clandestinely before the anthropologists who class Pithecanthropus and Sinanthropus as distinctive types, they could as consistently establish a half dozen or more racial types from them.

Within this Scandinavian stock is a man who is often referred to as the "Gorilla Man." The writer desired to see him. Finally the occasion presented

itself at a public gathering. From the description that had been given, his identity was at once revealed. But instead of being a gorilla man, he was a Neanderthal man. In most respects he was a good replica of some artist's reproduction of Neanderthal Man from data furnished by the anatomist. Here may have been a true descendent of traditional Neanderthal man stalking the earth today. Facial form, superciliary ridges, head build, posture of head on shoulder, forward stoop of massive shoulders, were of that description. Also within this Scandinavian stock occur many replicas of Cro-Magnon as traditionally described. The Norwegian stock, particularly, presents frequent examples of this conventionally typified man. Every considerable gathering of persons presents the same wide contrasts in anatomical traits. The riddle is how we come to consider them being racially alike at all rather than why we do not assign them to many different racial types.

The traditionally conservative attitude of anthropologists regarding our ancestry may be partly explained by their view of evolutionary methods. To think according to one supposed method of evolution might lead to conservatism, while a recognition of the operation of the other method would doubtless cultivate liberalism. It is interesting to note that anthropologists do not trouble to discuss methods of evolution, evidently regarding such matters as irrelevant or as having been settled by Charles Darwin.

It seems that most of our scientific thinking in the biological field regarding evolutionary development views evolution as natural selection of succeeding generations from a multitude of slightly varying forms. According to this mode, it might require millions of years for a series of slightly but gradually differing forms to grade upward into a new species. If the intervening gradient forms between species were not in some way preserved, there would be no record of the process and the connecting "missing links" between species would be lacking. There are not many cases where the record of gradual evolution has been as well preserved in nature as in the case of the horse, and even there wide genetic gaps appear. But it is one thing to hold that evolution of species has taken place in spite of the fact that the in-between links are missing and another to say that this species or form has not developed from that, no matter how much it seems such evolution must have taken place, because there is no step-by-step evidence of the fact. Thinking in this fashion, one is able to see how it is possible that "evolutionary anthropologists" build genealogical trees supposed to give the origin and development of man that consists of nothing but trunk and limbs with modern men perched as twigs at the top without any direct ancestors.

A widely different view of evolution makes room for the admission of large mutations, or variations from the preceding generation, in the developmental process. De Vries saw his primrose which he had watched for years all at once bear new species of flowers, the proof that they were new species consisting in the fact that they bred true to form without reversion to the old pattern of flowers. Clark, previously quoted, states that we find no transition links between the fundamental varieties of our dogs, although we know they have developed from one or a very few original types during their association with man. We can accept it as a fact (which Charles Darwin recognized) that large mutations, or leaps, in the form of organisms do occur among both plants and animals. By this means, some form of animal life may develop a new species of its kind during the span of one generation, whereas by the former method

discussed that result might require millions of years. By this means of mutation it is conceivable that some such pithecan form as Dryopithecus gave rise to the man-like ape, ***Australopithecus africanus;*** that that form at some time issued forth into some such primal human form as the ape-like man, Pithecanthropus or Sinanthropus; that from this beginning human sprang higher transitional humans, such as Heidelbergensis; that this level mutated into Neanderthal; and that finally Cro-Magnon was a mutation from Neanderthal. This mode of thinking is certainly as scientifically consistent as the other traditionally conservative one; and it has the advantage of accounting for man and for present men in terms of definite genealogical ancestry. Instead of standing forever as cousin or uncle or grandfather limbs on the genealogical tree, these enumerated beings would serve as ancestral forms linking us back through the various human levels to the pithecan strain from which humans and other anthropoids derived. Only by some such mode of thinking as this can the anthropological dilemma be solved and the puzzle of ancestorless man be replaced by scientifically derived genealogical forebears.

It is not necessary to think that a mutation of all anatomical features took place at one great leap; that conventionalized Neanderthal Man with all his anatomical peculiarities all at once dropped those features and mutated into the conventionalized Cro-Magnon Man. What would happen is a paring down of the most glaring features of the more subdued type of individuals and the consequent production of progeny which would approximate the Cro-Magnon variants nearest to the Neanderthalers. Such a transitional mutation would not be unthinkably pronounced.

The causes of mutations and variations may inhere in organic life or may be found in external conditions. The doctrines concerning small continuous variations are that they are inherently constitutional. Variability among progeny is a constantly recurrent fact; the higher the evolutionary level of the generation of progeny, the greater the variability. Consequently, aside from identical twins, we never find two puppies or two human babies exactly alike. This, it is held, gives nature a great opportunity to discover the fittest for its purposes, to favor the survival of those which are fittest, and to eliminate the unfit and the less fit. So evolution of species by natural selection and survival of the fittest is supposed to take place. Now in the case of large variations, which are referred to as mutations, some of the causes of variations, at least, are in the situation outside the organism. This is demonstrated by the fact that experiments in the field of radioactivity have produced new species among fruit flies and other lowly forms. The possibility of producing new types of humans by this means is unknown to the present writer and he does not know if it has been attempted. But it looks quite possible that mutation among higher animals, including man, may have taken place during past times. We know that radioactivity is present all the time on the earth and that it is more manifest in certain places than in others. There appears to be no insuperable objection to assuming that some higher organism living in a place of heightened radioactivity was influenced genetically, with the result that a new species was the outcome. This is hypothetical, of course, but it possesses a rational foundation. It is no more conjectural than the traditional view and it is more intelligible.

During past times and among lower levels of organisms, variations and mutations were demanded or justified by environmental conditions. This signifies that the environment or some environmental conditions placed a premium on those variations and mutations

which possessed heightened survival value in that particular milieu. Nature "favored" (permitted) the survival of such forms. Environmental "demand" (pressure) for such new forms amounted to no more than that, except, of course, in the case of stimulus by radioactivity. In that sense, only, was nature seeking new forms of life, bidding for them. They could survive because of their survival value, fitness, adaptability to live in terms of conditions nature prescribed and imposed. The first men stood a better chance of surviving than did their pithecan forebears just because they had developed more resources to circumvent and make use of natural conditions. Physical nature was the total environment, practically, to which they had to adapt themselves. So this natural environment, chiefly, eliminated or perpetuated them. For this reason a new and advantageous organic form for that physical environment had a chance to be discovered by the environment and to be favored (passively) for selection and survival.

Whether or not there will be future evolution of man revolves about the part environment plays in selection and survival. The crucial consideration is that "civilized" man today is surrounded by another environment than were beginning men and that the more we become urbanized and mechanized the more this is true. Culture, social environment, has largely thrust physical environment aside and constitutes the chief set of external conditions to which present men adjust themselves. Man's survival value now is rated in terms of socio-cultural conditions and standards of valuation instead of physical. During pre-cultural days nature might place a premium on physical size, strength, speed, endurance, so that the organisms best constituted in those respects were selected by nature to survive, were the fittest. But under urban and machine environmental conditions the possession of super height, super weight, super speed or other super physical attribute may not be an advantage, but sometimes even be a disadvantage. Our socio-cultural environment is not demanding mutations in those directions. We are producing mechanical devices which possess super qualities to a degree that no natural mutation could begin to compete with. Telescopes, microscopes, trains, autos, airplanes, diggers, pumps, cranes, looms, telephones, radios, loudspeakers, each in its way can outdo many fold anything that any new mutation in a human organism could possibly accomplish. In this sense, the environment today is not demanding, pressing for, placing a premium on the appearance of new, "improved" human mutants. Such mutations would have no or little survival value and consequently, if the new being did survive or were allowed to survive, he or it would possess little if any superior competitive advantages. It might even occur that because of his super height or weight or farsightedness or micro-vision he could only qualify as a "freak" and travel about with sideshows to be exhibited at conventionally small admission fees. Which, of course, means that if a marked mutant were born we would have no means of evaluating the departure from the normal, would not know it if it appeared, would not know what to do with it, would not "recognize" it as having any important significance. It would not be outstanding in our kind of socio-cultural environment until it had been cultured for many years. It might well get lost in the shuffle and perish.

Perhaps the only human mutation which might prove valuable would be one of brain. Whether humanity "needs" better brains is a disputable matter. Evidently better brains or better trained brains would be a distinct advantage to some people we know. But brain capacity is a matter of relativity in that it

varies by successive and innumerable gradations from the sheerest idiot to the most gifted thinker and inventor. We might agree that we need more geniuses and fewer idiots, if we did not stop to inquire what is meant by "need" and "for what" the need exists. What we would have to discuss is whether on the average we humans need more brains. If we took the average of all extant mental capacities, it would be the average (mean) capacity of all gradations of capacity from idiot to genius. The average capacity might be raised by either of two means: increasing the number (proportion) of geniuses or decreasing the (number) proportion of idiots. We do not at all know how to accomplish the former and only slightly how to secure the latter result. Left to spontaneous mutations, either consequence may ensue. If a transcendent genius by nature were born, he might never be touched (stimulated) by our educational process and might disappear unknown. Unless we could discover him at birth and raise and nurture him as a genius (something we are scarcely prepared to do), he would be likely to remain as one of the estimated fifteen undeveloped and unknown native geniuses among every sixteen that are born.

Were we to develop all our potential geniuses into operational geniuses and also develop all other grades of "normal" brains up to their maximum capacity instead of to the eighth grade level as at present, the resulting increased productivity which would be manifest all through society would be so startling that we would cease to speculate on our need for better brains.

HISTORY OF THE MEASUREMENT OF HEAT

II. THE CONSERVATION OF ENERGY

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THE development of quantitative thermal science may be thought of as comprising three broad stages: the first was the measurement, three hundred and fifty years ago, of heat levels or intensities through *thermometry;* this was followed about seventy-five years later by the determination of relative heat capacities or quantities through *calorimetry.* The third step was the correlation of the sciences of heat and mechanics which led a hundred years ago to *thermodynamics.*

It is the aim of science to bring all phenomena, including heat, into one unified and self-consistent system. Attempts to correlate heat with mass had been signal failures; but there was another linkage which had been more or less apparent since prehistoric days. Primitive men who struck a fire through friction were aware of the fact that kindling temperatures can be produced by motion. Conversely, the experiments of Philo and Hero had shown that motion can result from differences in temperature. It was the *quantitative* form of expression given to these two simple *qualitative* observations which initiated the third period in the science of heat and led to the law of the conservation of energy.

Conservation theorems are perhaps as old as science itself. *Nihil ex nihilo* has been accepted as more or less self-evident ever since the Ionian philosophers began their search for the underlying unity and permanence in this world of apparent multiplicity and ceaseless change. In Peripatetic science there was an understanding that the visible processes of generation and corruption were but conversions in form of a basic *prima materia.* In the atomic school of Leucippus, Democritus, and the Epicureans, the indestructibility of matter had been axiomatic. All changes were but the confluence and separation of primeval atoms. The eternity of motion as well as matter was implied by the teaching of the ancient atomists and by the medieval concept of inertia; but it was first given explicit expression in modern times by Descartes. In 1644 he held that the universe was a plenum to the matter of which God had in the beginning imparted a given totality of vortical movement. This motion endured eternally, because of the contiguity of the particles of matter throughout all space, and remained quantitatively invariant.

The philosophical principles of the conservation of mass and motion were paralleled by similar laws based upon mathematical and experimental physics. The law of the lever is a simple illustration of the mechanical rule that what is gained in power is lost in distance. That is, it represents an adumbration of the compensation idea or the law of the conservation of work. This general principle is implied also in the postulate of the impossibility of perpetual motion from which Stevin in 1586 deduced his principles of hydrostatics, the law of the inclined plane, and the idea of statical moment. Galileo saw the same principle in operation in the fact that a pendulum bob was found never to rise above the height from which it had fallen. Torricelli and Huygens stated more generally that the common center of gravity of a

system of bodies cannot of itself rise above the height from which it fell. From the observations of these men the concept of work and the law of the conservation of mechanical energy in an isolated system gradually were crystallized out during the seventeenth century.

For almost a century following the death of Descartes there raged between the Cartesians and Leibnizians a controversy arising from an apparent inconsistency between the philosophical doctrine of the conservation of motion and the mathematical definition of work. The term motion at that time was understood to mean the product of mass and velocity, or what now is called momentum. On examining the matter closely, Huygens concluded that Descartes had been wrong. From the concept of work it followed that in certain cases of elastic impact the sum of the momenta after impact was definitely not equal to that beforehand. Had Huygens in these cases subtracted rather than added the momenta, he would have found that the principle of Descartes was indeed justified if corrected to state that the total momentum *in any given direction* is the same after impact (elastic or inelastic) as before. This law of the conservation of directed momentum or of mechanical effect is, in fact, essentially equivalent to Newton's third law of motion. Although Huygens overlooked the possible correction of Descartes' law, he discovered about 1669 the striking fact that if one multiplies each mass by the *square* of the velocity, rather than by the first power, then in all cases of perfectly elastic impact the sum of these products remains the same after impact as it was before. This focussed attention upon a new entity—the product of mass and the square of velocity, or what Leibniz called *vis viva*. This is essentially what is now known, in the form $\frac{1}{2}mv^2$, as kinetic energy.

Meanwhile it had been recognized that mv or momentum was equivalent to the product of force and time, whereas $[\frac{1}{2}]mv^2$ or *vis viva* was equivalent to the product of force and distance. Cartesians maintained that the efficacy of a force was measured by the time through which it acted; Leibnizians insisted that the efficacy was a function of the distance. Two hundred years ago this famous controversy came to an end when D'Alembert in 1743 showed that the dispute was merely one of terminology. The efficacy can be measured in terms of the product of force by either time or distance, the difference lying only in the units chosen. Mechanical energy, whether measured as momentum or as kinetic energy, is always conserved under perfectly elastic impact, as the so-called principle of D'Alembert indicates.

But what of inelastic impact? Leibniz about 1686 had sought to generalize Huygens' result for this case also and to establish it as a cosmological doctrine. He saw that *vis viva* was essentially equivalent to work, and that possibly it existed also in other forms as a sort of potentiality. The terms kinetic energy, work, and potential energy now have precise and distinctive meanings, but Leibniz referred rather loosely to all three when he spoke of the great universal law of the conservation of force or *vis viva.* This law he regarded as justified by a general equality principle, *Causa aequat effectum,* which is somewhat equivalent to Newton's law of action and reaction, published in 1687.

For almost half a century the law of Leibniz went more or less unnoticed, possibly because Newton cast doubt on its validity. Nevertheless, in 1731 Christian Wolff asserted again that *vis viva* remains constant in *all* cases of impact. In 1735 Jean Bernoulli discussed the nature of force and concluded that it was something real and substantial, and hence must be invariable in quantity. In cases of inelastic impact he believed that any apparent loss of *vis*

viva corresponds, because of the equality of cause and effect, to some form of potential energy, such as a force of compression of the bodies. The idea of potential energy had been adumbrated by Gassendi and Borelli, and had been expressed somewhat more definitely by Leibniz. Daniel Bernoulli, son of Jean, in 1738 gave a clear distinction between actual and potential motion—or between kinetic and potential energy—in the principle which bears his name. This principle, like that of D'Alembert, is equivalent to the conservation of mechanical energy for perfect machines; but Daniel Bernoulli held that the law of the conservation of *vis viva* was valid for all situations, terrestrial and celestial. It is indeed surprising that he did not anticipate the general conservation of energy more definitely. He adopted the idea of internal energy and the kinetic theory both of gases and of heat. Nevertheless, although he expressed the belief that a cubic foot of coal contained the work-equivalent of 8 to 10 men for one day, he did not calculate a definite mechanical equivalent of heat.

In 1742 Voltaire's mistress, the Marquise de Châtelet, likewise expressed the compensation idea. She admitted that in cases of inelastic impact it is difficult to follow the course of the *vis viva,* and that some appears to be lost. Nevertheless, she was quite certain that the force had not in reality perished. Such confidence, however, failed to convince others, and the law was largely forgotten until precisely a century later when Mayer showed clearly that it was justified. Any apparent loss is the result of a conversion of *vis viva* into some latent form which, as Daniel Bernoulli had suspected, in many cases is nothing but heat.

The one-hundred-year delay in the establishment of the law of the conservation of energy is striking. The difficulty with all the enunciations of the law put forth between 1686 and 1742 was that they were at best bold extrapolations beyond experimental evidence which were justified by faith in the unity of nature. Only in cases of perfectly elastic impact had Huygens shown that *vis viva* remains constant. D'Alembert in 1743 warned against the metaphysical point of view which would make a primitive universal law of nature out of something holding only in certain definite cases. His warning was observed for almost a century. Then Mayer in 1842 boldly repeated *Causa aequat effectum.* In 1843 Joule similarly asserted that "the grand agents of nature are by the Creator's fiat, indestructible"; and Colding in this same year maintained that "force is a thing, imperishable and immortal." In such metaphysical terms did these men re-enunciate the law of the conservation of energy. However, D'Alembert himself would have excused them, for their pronouncements were accompanied by what had heretofore been lacking—precise quantitative experimental evidence. But to understand their work it will be necessary to return to the question of the nature of heat.

On numerous occasions, particularly during the seventeenth and eighteenth centuries, it had been suggested that heat was the result of a rapid motion of the parts of the matter affected. Until 1798, however, no one had been able to show that heat was indeed measurable in terms of momentum or kinetic energy. In that year Count Rumford reported on his spectacular and classic experiments at the Munich arsenal on the heat generated during the boring of cannon. That very high temperatures could be obtained through friction was a commonplace known even to neolithic man. This phenomena was readily explained by theoretical science as due either to the liberation of caloric from the abraded materials (or perhaps from the surrounding atmosphere) or else to the generation of vibrations among the particles of the substances. That is, the

materialistic theory found the source of heat *within* the substances and looked upon the frictional motion as simply the agent which converted this internal latent heat to sensible heat; the dynamic theory, on the other hand, interpreted the change in temperature as the conversion of *external* mechanical energy to an increased internal energy of vibratory motion. Either explanation was *qualitatively* satisfactory. Rumford, however, noted a strong *quantitative* argument against the caloric theory. The source of the heat generated by friction in these experiments appeared evidently to be inexhaustible! The equivalent of 26.58 pounds of ice-cold water had been made to boil in $2\frac{1}{2}$ hours by the friction produced by machinery which could easily be powered by one horse. Given sufficient time, an indefinitely large amount of heat could be engendered. Moreover, calorimetric tests showed that there had been no perceptible loss of heat capacity on the part of the metal from which the heat came. "It is hardly necessary to add," said Count Rumford, "that anything which any insulated body, or system of bodies, can continue to furnish without limitation, can not possibly be a material substance." Rumford suggested that heat was rather a condition of bodies—a mode of motion.

Rumford's apparatus was not in reality completely insulated, for it remained in contact with the air. Moreover, the constancy of the heat *capacity* of the metal chips did not prove that a constant *quantity* of heat was retained. Hence his work was not thoroughly convincing. The following year Davy gave further evidence that heat is not matter. He showed in 1799 that two pieces of ice might be melted simply by rubbing them together vigorously. In this case also, however, it could be argued that the heat necessary to melt the ice somehow had come, not from the frictional motion, but from the air. Davy therefore performed a second experiment in which two pieces of metal in contact with wax were mounted on ice and rubbed together by clockwork in a receiver which had been exhausted by an air pump. In spite of the effort to remove every possible source of heat, the friction here produced a rise in temperature sufficient to melt the wax. Davy concluded from this that friction does not diminish the capacity of bodies for heat; but that heat may be defined as "a peculiar motion, probably a vibration, of the corpuscles of bodies, tending to separate them."

The experiments of the young and inexperienced Davy did not carry conviction. Although the vibration theory in 1807 was accepted by Young (who first substituted the word energy for *vis visa*) and somewhat later by Ampère, the caloric doctrine continued to predominate for another half century. One reason for the delay in the acceptance of the kinetic theory of heat may be found in the fact that before 1842 no precise and explicit conversion figure was given for thermal and mechanical energies. From Rumford's data one can indeed calculate, on the basis of Watt's estimate of one horsepower as equivalent to 33,000 foot-pounds per minute, a mechanical equivalent of heat of 1034 foot-pounds per British thermal unit. However, the idea of a constant proportionality factor in the conversion of work into heat is more implied than expressed in his account. Rumford did not pursue the theoretical implications of his experiments and left unanswered the knotty inverse question of the quantitative convertibility of heat into mechanical effect.

The problem of converting heat into work had up to this time remained largely in a qualitative stage. The contrivances of Philo and Hero were primitive means of achieving such a conversion, but no attempt was made to measure the heat expended or the work done. The improvement of these devices by Porta, de Caus, Branca, the Marquis of Worcester, Savery, Papin, and New-

comen resulted in more practical heat engines; but Watt saw that they were still exceedingly wasteful of fuel. Watt's mechanical ingenuity enabled him in 1769 to patent a machine with a separate condenser which was so great an improvement over earlier forms that often he is regarded as the effective inventor of the steam engine. Moreover, he was unusually sensitive to the need for precise measurement. He gave definite numerical significance to the term horsepower; he discovered the quantitative composition of water independently of Cavendish and Lavoisier; and he was inspired by Black to make careful determinations of specific and latent heats. Nevertheless, it remained for Sadi Carnot to establish in 1824 the quantitative theory of the engine which Watt had improved.

Carnot's thought was influenced to a large extent by Fourier's mathematical analysis of thermal conduction and radiation. Fourier had remarked, as had Lambert a half century before, that differences in temperature were somewhat analogous to differences in water level in that the work which could be obtained from the system depended both on the difference in level, or potential, and on the quantity. This would seem to imply that with heat, as with water power, the quantity of the working substance is the same at the end of operations as beforehand. Carnot accepted this conclusion and in his early work looked upon heat as material. Work was not the result of a conversion or loss of heat, but was due wholly to letting caloric down from a higher to a lower temperature or potential. He found, however, that the quantity of work was not directly proportional to the difference in potential. His calculations showed that motive power is given in terms of temperature by a function according to which the efficiency drops off with an increase in the temperature of the condensor or sink. This observation later became the basis for the *second* law of thermodynamics, but Carnot's early materialistic views obscured the way toward the *first* law. Carnot computed that 1.12 units (kilogram-meters) of work were furnished when 1000 units (kilocalories) of heat passed from 100° to 99° C. If one were to read into Carnot's work the ideas of entropy and the dynamic theory of heat, this estimate would give a mechanical equivalent of about 418 kilogram-meters per kilocalorie; but Carnot did not interpret his calculation as a conversion of heat into mechanical effect, for he thought of the quantity of heat as unchanged. Soon after 1824, however, Carnot seems to have become a convert to the mechanical theory of heat. In an undated manuscript of this period he said: "Motive power is in quantity invariable in nature; it is, correctly speaking, never either produced or destroyed." Moreover, this clear enunciation of the conservation of energy differed from statements of a century before in that it included the first precise value for the mechanical equivalent of heat. Carnot calculated from the specific heats of air that the creation of a unit of motive force resulted from the destruction or conversion of 2.70 units of heat. This figure is less accurate than that implied by the work of 1824, but it was based upon the modern view of heat and energy. Unfortunately, Carnot's premature death in 1832 prevented him from elaborating on the implications of this work and from publishing an account. It remained unknown until the brief manuscript note was discovered and published about a half century later. Meanwhile, there were in the decade from 1837 to 1847 no fewer than half a dozen men who, quite independently of each other, pursued the same line of thought and share in the discovery of the conservation of energy.

Speculation on the general conservation of mechanical effect, latent as well as patent, had been largely abandoned

about a century earlier. In the case of frictionless machines the idea of compensation, or the conservation of work, continued to be accepted as axiomatic in mathematical treatments of mechanics; and the Paris Académie by 1775 had decided to reject all papers on perpetual motion. Work done against friction, on the other hand, was regarded as in a sense wasted, lost, and destroyed. However, during the early nineteenth century evidence from new sources pointed to closer quantitative interrelations between natural phenomena. Galvani and Volta had shown, just before the century opened, that chemical forces were convertible into a continuous electrical current; and Nicholson and Carlisle in 1800 had indicated the converse. Then Oersted in 1819 disclosed that galvanism and magnetic forces can generate motion; and Faraday in 1831 discovered inversely that motion and magnetism can produce current electricity. In 1833 John Herschel pointed out that solar influence was indirectly the source of all motion on the earth. Such disclosures led Mohr in 1837 and Grove in 1842 to assert that motion, heat, light, chemical affinity, electricity, and magnetism are but different forms of force or energy. They are mutually dependent and when one form disappears another appears to take its place. Mohr suggested, but did not complete, a calculation of the work-equivalent of heat from the specific heats of air.

Two years later Séguin studied the steam engine in order to measure the difference between the heat which had left the boiler and that which reached the condenser. Having adopted the mechanical theory of heat from his uncle, the famous balloonist Montgolfier, he maintained that the heat lost during the expansion of steam is necessarily equivalent to the work done during this expansion. In 1839 he gave data from which the mechanical equivalent of heat can be calculated. In 1847 he made an explicit calculation and arrived at about 449 kilogram-meters; but by then he had been anticipated by Mayer, Colding, and Joule.

In 1840 Mayer journeyed to Java as surgeon on a Dutch vessel. In bleeding patients he was surprised at the bright red color of venous blood of men in the tropics. He concluded, on the basis of Lavoisier's work, that this was due to a lower metabolic rate in torrid zones which called for a smaller consumption of oxygen and resulted in less color contrast between venous and arterial blood. He came to realize more keenly the relationships between food, heat, and work. Mayer was convinced, on the basis of metaphysical principles, that heat and work are qualitatively different forms of something which is quantitatively indestructible. He was aware that such a general principle would have to be supported by very definite empirical evidence before it could meet with the approval of critical scientists. Mayer was lacking in mathematical and experimental technique, but he adopted the method which Mohr earlier had suggested for calculating, from calorimetric data well known to the world of science, the mechanical equivalent of heat.

Mayer maintained that the heat evolved when air is compressed is the dynamical equivalent of the work employed in compressing it. On this basis he made the assumption, later fully justified by the experiments of Joule, that the specific heat of a gas at constant volume exceeds the specific heat at constant pressure by a quantity of heat equivalent to the work which the gas in the former case will do if allowed to expand to its original pressure. On carrying out the necessary calculations, Mayer concluded that 1 unit (calorie) of heat will raise 1 gram about 367 meters.

Mayer was not first in enunciating a general principle of the conservation of energy, nor was he first in calculating the mechanical equivalent of heat. He

is, however, entitled to priority as the first person to *publish* a clear, explicit *statement of the principle* together with a precise and reasonably accurate *value of the mechanical equivalent* derived from experimental data. Nevertheless, there were others who must be recognized as independent co-discoverers. Colding, for one, was led to similar ideas at roughly the same time. About 1839 he was puzzled by a study of D'Alembert's principle of active and lost forces. He concluded that, inasmuch as the forces of nature are akin to the intellect in being something spiritual and immaterial, they ought to be regarded as absolutely imperishable. Therefore, "when and wherever force seems to vanish in performing certain mechanical, chemical, or other work, the force then merely undergoes a transformation and reappears in a new form, but of the original amount." Whereas Mayer had recourse to the scientific data at hand in calculating the mechanical equivalent of heat, Colding collected new data from a variety of experiments on the heat of friction. From some two hundred measurements he arrived at a figure of about 350 kilogram-meters; but he was encouraged by Oersted not to put his idea before the Royal Society of Science at Copenhagen until he could give an experimental demonstration of it. Hence, his "introductory" presentation was delayed until 1843, at which time similar conclusions of Joule, based upon more complete and accurate experimental data, obscured Colding's achievement. However, Joule's work likewise had been delayed by his observance of Herschel's advice that "hasty generalization is the bane of science."

While Colding was pondering over the principle of D'Alembert, and Mayer was on his way to Java, Joule presented his first papers on the relations between chemical, electrical, and thermal energy. Faraday's laws of electrolysis had shown that chemical affinity and electromotive force are quantitatively related. Joule extended this to show that *chemical and electrical energy are quantitatively equivalent to the heat produced* in the electrical circuit, both in conductors and in voltaic and electrolytic cells. But Faraday in 1831 had shown that electrical currents could be produced mechanically as well as chemically. Joule saw that the heat produced by a current from a dynamo should be the same as the heat of friction which would have been generated directly by the force operating the dynamo if it had not first been converted into an electric current. Joule therefore measured the work done in producing a current through electromagnetic induction and calculated the *mechanical equivalent of electrical energy*. Then through his previous work on the *electrical* equivalent of heat he deduced as the *mechanical* equivalent of heat the value 838 foot-pounds per British thermal unit.

The work of Joule stands in sharp contrast to that of Mayer. For Mayer the conservation law had been in the nature of a sudden intuition, or at best a philosophical discovery supported by a somewhat slender bit of calculation; for Joule it represented an inductive inference justified by a wealth of accurate data derived from a variety of experiments skilfully devised and patiently executed. Joule was not satisfied to determine the mechanical equivalent of heat from one experiment or even from a single series of experiments. As a postscript to his paper of 1843 he supplemented his mechanical-electrical-thermal calculations by a method eliminating the electrical step. By forcing water through fine tubes—Carnot had suggested this method in his unpublished manuscript—Joule found directly an equivalent of about 770 foot-pounds. Two years later he deduced the value 798 through the heat disengaged and the work done on compressing air. From 1845 to 1847 he carried out his favorite method on the fric-

tion of liquids produced by paddles and falling weights, obtaining about 782. His final estimate of the mechanical equivalent derived from all of his work was 772 foot-pounds.

Before 1847 the law of the conservation of energy had been independently adumbrated in forms of varying degrees of accuracy and generality by Rumford, Carnot, Mohr, Séguin, Colding, Mayer, and Joule. Yet the principle of conservation neither carried conviction nor was widely known. The task of making it scientifically acceptable was reserved for still another discoverer, von Helmholtz. He was a physiologist who, like Galvani and Volta, worked on the muscles and nerves of frogs' legs. Helmholtz wished to banish from biology the concept of vital force and so sought to measure the heat produced in muscles during chemical changes. From such studies he was first led to the conservation of energy. However, the many-sided Helmholtz was also a physicist and mathematician, and so he sought to establish the law upon a sound postulational basis similar to Lagrange's treatment of mechanics. He found the necessary first principles in the work of Stevin and Newton—in the impossibility of a perpetual motion and in Newton's third law. Through an elaborate mathematical discussion he showed that all the known cases of the transformation of energy could be traced back to these principles, and from them Helmholtz deduced the law which he called *The Conservation of Force.*

The law of the conservation of energy states that in any inter-transformation of heat and molar motion (or of any two types of energy), the amount of that form of energy which disappears is exactly equivalent to the amount of the other form which is created. In this respect the two forms are on the same basis. However, Carnot had found that motion and heat are not mutually and completely interchangeable. Given a quantity of mechanical energy, it is indeed possible to convert this fully and completely into heat. However, the converse can never be true, and it is this fact which at first prevented Kelvin and others from accepting the full significance of Joule's work. At best only a fractional part of a given preassigned quantity of heat energy can be converted directly or indirectly into work. At worst none may be so converted, as when the temperature of a free bar, the ends of which are unequally heated, is allowed to become uniform. This had been pointed out by Carnot in his classic work of 1824. Carnot based his reasoning on the idea of heat as an indestructible fluid, but his arguments and conclusions hold also, *mutatis mutandis,* for the dynamic theory of heat. Carnot saw that if the working substance can be brought back to its initial state, one will be in a position to tell precisely how much heat and work have been involved. This led him to the study of the so-called Carnot cycle. On the basis of this cycle and the impossibility of perpetual motion, he realized that no heat engine can be more efficient than one which is reversible. In all other cases, although there is no loss of energy, only a portion of the heat is convertible into work. Carnot thus recognized essentially the operation of the second law of thermodynamics, and the expression $(T_2 - T_1)/T_2$, which determines the convertible fraction, is now appropriately known as "Carnot's function"; but at the time the implications of his work were overlooked.

During the early years of the second half of the nineteenth century it was recognized by Clausius and Kelvin that the Carnot efficiency function has far-reaching implications. No known process in nature is exactly reversible and hence during *every* transformation of heat into work some heat energy, while not destroyed, is nevertheless rendered unavailable. Temperatures tend to be equalized, and work can be obtained from

heat only when there exists a difference in potential or temperature. Although the total *energy content* of the universe remains the same, yet the amount which is *available* tends constantly to diminish. This recognition led to the pronouncement by Kelvin and Clausius that unless some other force intervenes, the universe is approaching a "heat death" in which there will be no differences in temperature, and hence no energy available for work, activity, and life.

The law of the conservation of energy probably did more than anything else to establish the dynamic theory of heat. Yet as Mach showed, the laws of thermodynamics are not necessarily inconsistent with other views of the nature of heat. Carnot, in fact, had practically anticipated the first two laws on the basis of a material theory. Such doubts as remained with respect to the mechanical view of heat disappeared about the middle of the last century. One reason for this was that atomic and molecular theory was then firmly established in chemistry. Moreover, successful physical studies of the internal forces which constitute heat already had been made—at least for the less complicated case of the gaseous state of aggregation. Daniel Bernoulli had advanced the kinetic theory of gases from a quantitative point of view, but his work of 1738 was largely neglected until revived and extended by Le Sage and Prévost (1818), Herapath (1847), and Joule (1848) to include the calculation of the velocities of the molecules for various temperatures and the determination that the heat capacity of a gas is given directly by its *vis viva.* In the next few years Rankin, Clausius, and Kelvin showed that intramolecular forces also must be considered a part of the phenomena of heat; and Clausius (1859) and Maxwell (1860) extended Joule's calculations to include the mean free path, the distance between centers at collision, and the number of molecules per unit volume. Such *quantitative* expression constituted confirmation of the kinetic view which was more convincing even than the striking *qualitative* evidence presented in 1827 by the Brownian movement. After this work chemists accepted atoms and molecules as real and substantial; and physicists by that time were thoroughly convinced that it is the motion of these particles which produces the sensation known as heat.

* * * * *

Twenty-five hundred years ago Pythagoras uttered the dictum, "All is number." This was reaffirmed by Plato in the words, echoed in our day by Whitehead, "God is a geometer." Science has come to reject the Pythagorean-Platonic teleological form of such a view; but it has found ever greater confirmation of the corollary that "to measure is to know." In this respect the science of heat has been far from exceptional. A brief review of such quantitative aspects as thermometry, calorimetry, and thermodynamics does not indeed exhaust the subject of the theory of heat; but further investigation into these and other branches will amply confirm the impression which such an elementary review has afforded. Here, as in other fields of science, it was an insistence upon quantitative methods which made possible the development in theory. Qualitative notions on the nature and properties of heat began to take on significance only when, three hundred and fifty years ago, a crude instrument was devised to give them quantitative form. Two and a half centuries later thermometry gave rise to what was at the time the greatest unifying principle of all science. There was in this dynamic changing universe one entity which remained eternally the same—the quantity of energy. As science goes on in the search for still greater unity, let it be remembered that the way is paved, not with unmeasured speculation, but with the objective data of patient quantitative research.

WATER—THE UNIVERSAL ADULTERANT

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Water is a vital necessity in our social life, and for our physiological processes. We can abstain from food for a much longer time than from water and still live to tell the tale. Without sufficient water our crops would fail, our animals used for food would die, and the human animal would perish from hunger and thirst. Wars have been waged over water holes. Cities, states, and nations have spent, and will continue to spend, vast sums in order to deliver to their citizens an abundant supply of pure, fresh water.

Water has many commercial usages. It is used for the production of power; it is extensively used as a cleaning agent. It is used as a solvent in the household, as well as in the arts. Its application as an adulterant of many articles of merchandise depends largely upon its unique solvent properties. In fact, water, in addition to being the universal solvent, may well be considered as almost the universal adulterant, and its use as an adulterant is responsible for the term "watered stocks." The detection of added water is often difficult because of variance in the natural moisture content of the substances which are thus adulterated.

Foods

Milk. The watering of milk is historic, prehistoric, and no doubt will continue as long as the human race uses milk and desires an excess profit.

The watering of milk in Massachusetts was formerly more prevalent than at present. Its reduction has been brought about by the discovery of methods for the detection of this form of adulteration, and by the imposition of substantial fines. The practice, now so frequently used, of buying milk by "weight and test" is also responsible for this reduction. It is not profitable to water milk when it is sold on a 3.7 per cent fat basis.

Average market milk is 87.4 per cent water, but natural milk will vary between 83 and 90 per cent water. The addition of 15 per cent of water to average market milk will increase the water content to 89.2 per cent which is less than the water content of certain natural milk.

A small amount of added water may be detected by the freezing point of milk, or by the determination of the refractive index and ash of the milk serum. A larger quantity may be detected by the specific gravity and sometimes by the taste of the milk. A much larger quantity may be detected visually. The quantity of watered milk sold is extremely small relative to the total milk supply.

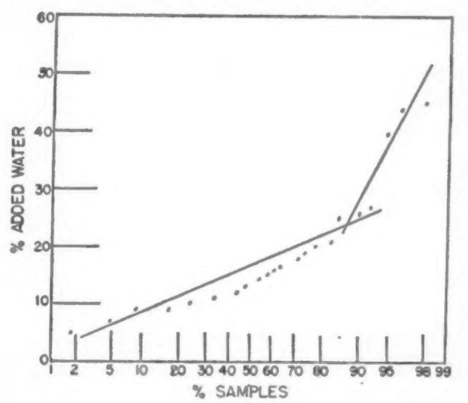


FIG. 1.

In order to show the variation of added water in adulterated milk, the records of the year 1925 were selected.

During that period 6,813 samples were collected and 150 samples of watered milk were obtained from fifty-three persons, from one to eight samples being taken simultaneously from each person. The median analysis of the samples taken from each person represents the fifty-three analyses reported upon Figure 1 with arithmetic probability scales. These figures plot as two probability series. One series represents ninety per cent of the samples which were obtained from those who desired to play a more or less reasonably safe game, and the other ten per cent was obtained from those who watered the product they were selling without regard to the possibility of detection and subsequent litigation.

During 1942, 5,649 samples of milk were collected, of which fifty-one contained added water. Eight of the nine persons responsible were prosecuted and convicted. The ninth person was out of our jurisdiction, the milk having been shipped into Massachusetts from another state.

Butter. Butter is a mixture of milk fat, water, casein, salt, lactic acid, and color. Of these substances, fat is the most expensive and water the cheapest. After the passage of the United States Food and Drug Law of 1906, the Secretary of Agriculture, under the provisions of the Act, adopted a minimum standard of 82.5 per cent butter fat. Unfortunately, the United States Law of 1906 provided no penalty for non-compliance with the standards.[1] Most of the states adopted this United States standard, the state laws providing penalties for violations. Non-compliance with the standard began to be the rule, and appeals to the United States Department of Agriculture for action in interstate shipments of low standard butter were often ignored. Massachusetts was informed that no action would be taken under the United States law unless fat was less than eighty per cent *and* water above sixteen per cent. The following is quoted from the 1921 report of R. O. Baird, Chief Deputy Food Commissioner of North Dakota:

> The standard for butter which is used as a guide by the federal and most of the state and city food officials requires that butter shall contain not less than 82.5 per cent milk fat. This leaves 17.5 per cent for salt, curd, coloring matter, and moisture. On the average something less than 16 per cent will represent moisture.
>
> The federal officials do not recommend seizure when the butter contains as much as 80 per cent milk fat and is otherwise in accordance with the law.
>
> The sale of water and salt at the price of butter is not only a fraud upon the consumer and an unnecessary burden upon the public, but is also demoralizing to the butter industry.
>
> The man who puts excess water in his butter can cut the price sufficiently to get the business and still make more than a fair profit. The cut price, however, seldom reaches the consumer, since the additional water is not apparent and the butter usually is sold at regular market prices. As a rule, the manufacturer and the dealer thus divide between them the profit on water sold as butter.

The literature shows that the moisture content of butter is quite variable. Konig reports 317 analyses made between 1864 and 1895, showing moisture variation from 4.15 per cent to 35.12 per cent and averaging 13.45 per cent. The United States Bureau of Animal Industry published, in 1912, 692 analyses of butter with moisture varying from ten per cent to eighteen per cent, with a median of fifteen per cent. The Massachusetts Department of Public Health in 1923 and 1924 examined 502 samples of butter, some of which had been in storage for nearly a year, the moisture of which varied from 8 per cent to 18.5 per cent, averaging 14.7 per cent. A report on New Zealand export butter gives 828 analyses above 16 per cent moisture, averaging 16.38 per cent, and 144,952 analyses below 16 per cent moisture, averaging 15.04 per cent. moisture.

[1] The new Food, Drug, and Cosmetic Law provides a penalty for violating the standards and regulations of the U. S. Food Administration.

In 1940, C. S. Ladd, then North Dakota Food Commissioner and State Chemist, published the results of the analyses of 513 samples of butter to be sold under the 80 per cent fat standard enacted by Congress in 1924. The following table gives a summary of the results:

Analyses of North Dakota Butter in 1921 and 1940

	Moisture		Fat	
	1921	*1940*	*1921*	*1940*
	%	%	%	%
Lowest	12.3	12.7	67.7	76.9
Lower quartile	14.0	15.5	78.9	80.3
Median	15.0	15.9	80.3	80.8
Average	15.3	16.0	80.1	80.9
Upper quartile	16.2	16.4	82.0	81.2
Highest	29.5	19.7	85.0	85.3

It is evident that a reduction in the standard did not cause any reduction but rather an improvement in quality, although it resulted in a reduction in law violation.

The art of manufacturing butter has progressed towards the elimination of excess moisture as well as low moisture; and it is progressing towards the production of butter less variable but relatively higher in moisture.

It should not be assumed that the federal authorities are lax in the enforcement of the fat standard for butter. The notices of judgment of the United States Food Administration frequently report prosecutions as well as seizures for selling butter containing less than eighty per cent fat.

Condensed Milk. Forty years ago condensed milk was more condensed and consequently contained less water. Standard brands contained from nine to twelve per cent fat and twenty-eight to thirty-seven per cent milk solids. Condensed milk now upon the market very closely approximates the standards of not less than 25.5 per cent solids, 7.8 per cent fat for the unsweetened variety; and 28.8 per cent milk solids, 8 per cent fat for the sweetened variety. The high concentration of the milk formerly on the market caused a crystallization of some of the milk sugar, which many purchasers insisted was sand. The lower concentration prevented this crystallization, and purchasers are now satisfied with the product. This reduction in concentration can hardly be considered as adulteration.

Cheese. The moisture content of food can be somewhat increased by adding a substance capable of absorbing many times its weight of water. A few years ago the water content of cream cheese was increased by this means. Some manufacturers added certain vegetable gums and sold the diluted product in competition with other manufacturers not using these gums. At the instigation of the manufacturers a change was made in the Massachusetts law permitting the practice of adding gums to cream cheese, and specifying a maximum moisture content of fifty-six per cent and a minimum fat content of seventy per cent on the moisture-free basis. Even now some manufacturers violate this standard which was made according to their own specifications. The Federal Food Administration by regulation now permits the addition of gums to cream cheese, and has prescribed moisture and fat standards for that article. The reason given by the trade for the addition of gum to cream cheese was to "prevent leaking."

Sausage. Another article of food containing more or less added moisture is the sausage. It seems that water is a necessary ingredient in its manufacture. The Supreme Court of Michigan, after considering extensive evidence, rendered a lengthy decision stating in part, "(a) Water is an essential ingredient in the manufacture of sausage, whether made with or without cereal." The Court also

stated, "It is conceded that the use of cereal requires more water than does sausage made from meat alone. Anyone of intelligence would, upon reflection, know this to be a fact." The addition of large quantities of water, however, is prevented by placing a limit (two per cent in Massachusetts) upon the content of "cereal or vegetable flour or any product thereof." Starch will absorb four times its weight of the water in which it is cooked. Starch is sometimes added to pork sausages and since these articles are not cooked prior to sale, the starch will not carry excess water. In this instance, the starch is added for the purpose of absorbing the excess fat during the cooking process, and the consumer eats a higher fat, lower protein, pork sausage than if he purchased a somewhat more expensive sausage containing less fat, more protein, and no starch.

Vukenack and Sendtner, interested in the use of color as a means of increasing the fat and reducing the proteins, report forty-nine analyses of sausages averaging as follows:

ANALYSIS OF SAUSAGES

No. of samples	*Character*	*Water*	*Fat*	*Protein*
		%	%	%
8	Uncolored mettwürste	35.4	40.8	19.0
8	Colored mettwürste	33.6	48.3	14.4
9	Uncolored cervelatwürste	24.2	45.9	23.9
6	Colored cervelatwürste	22.8	51.8	19.3
5	Uncolored salamiwürste	17.0	48.4	27.8
6	Colored salamiwürste	16.2	54.1	23.3
4	Uncolored rindfleischwürste[2]	48.0	27.0	20.3
3	Colored rindfleischwürste	50.6	26.0	19.6

The German sausage of 1899 was fairly dry. The A.O.A.C. formula for added water gives negative values in each instance from the above figures. M. Kreis reports the analyses of twenty-one Swiss sausages in 1907 as follows: Water forty-six to forty-nine per cent; fat twelve to twenty-five per cent; fat-free flesh eighteen to thirty per cent. John P. Street reports the moisture content of fifty-one samples of pork sausage examined in 1909 as follows:

MOISTURE CONTENT OF PORK SAUSAGES

Samples	*Moisture %*
18	32 to 39
26	40 to 47
3	48 to 55
4	56 to 58

Analyses of frankfort and bologna sausage made by this Department in 1943 showed moisture variation from 49 to 61 per cent, averaging 54.9, and protein from 12.3 to 14.8 per cent, averaging 13.7. Forty per cent of these samples, some of which contained soy bean meal, showed added water by the A.O.A.C. method.

Kichton reports some interesting experiments where the same meat mixture was mixed with varying amounts of water with and without the addition of starch. After cooking for fifteen minutes, sausage with no added water contained 51.5 per cent moisture; with 10 per cent added water it contained 53.7 per cent moisture; with 10 per cent added water and 2 per cent starch it contained 56.7 per cent moisture. Another series shows with no added water, 44.1 per cent moisture; with 30 per cent added water, 52.5 per cent moisture; with 30 per cent added water and 3 per cent starch, 59 per cent moisture. The figures from both these series are shown in Figure 2 on arithmetic logarithmic scales. The lines representing the material containing starch are not parallel, that relating to the 3 per cent starch

[2] Beef sausage.

mixture showing a more rapid increase in moisture as the added water is increased than does that relating to the 2 per cent starch mixture.

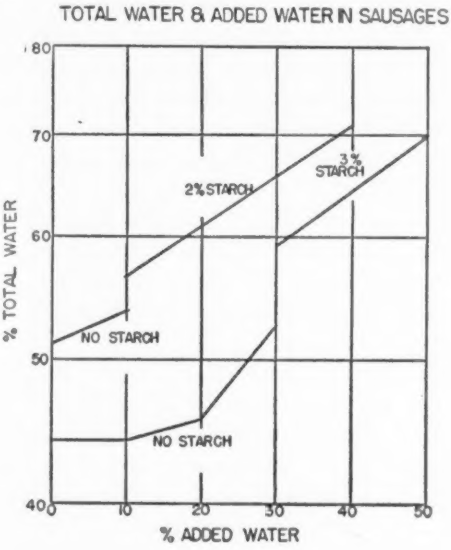


FIG. 2.

The following statement from a recent patent may be of interest. "There is introduced into the filler of a sausage casing a minor proportion of an edible water-absorbent vegetable gum such as agar-agar or gum arabic sufficient substantially to obstruct egress of moisture from the casing during hot processing and to jell the free moisture residual in the casing when the sausage is cooled so as to avoid wrinkling."

Hamburg Steak. Hamburg steak should preferably be ground by the consumer in his own home for, in this way he avoids the possibility of hamburg from more or less decomposed meat or hamburg forced through a dirty grinder. He avoids the addition of pork and thus is not likely to acquire trichinosis if he prefers his hamburg rare; and he also avoids the sulfited variety which is likely to carry from five to ten per cent water. Sodium sulfite is too slightly antiseptic to be of any practical value, but it possesses substantial deodorizing properties.

Beverages

Soft Drinks. Carbonated non-alcoholic beverages are mixtures of carbonated water, sugar, vegetable acids, color and flavor. Color and flavor constitute but a fraction of a per cent of the total material. Sugar is quantitatively the most costly of the ingredients. Since there are no standards for the sugar content of soft drinks, the variation is considerable in materials of similar character put out by different manufacturers. There is also a variance in the sugar content of soft drinks of different flavors. Ginger ale, sold under the paradoxical term "dry" ginger ale, for example, has a lower sugar content than other ginger ales and is consequently wetter and often better.

The Massachusetts Department of Public Health, several years ago, examined several hundred samples of soft drinks. Excluding the samples found to contain saccharine, the variance in sugar was from seven to seventeen per cent, the average being 11 per cent. During 1942, the sugar content of one hundred twenty-six samples of carbonated beverages varied from 11.3 per cent to 12.7 per cent averaging 11.9 per cent. Notwithstanding the recent sugar shortage, there was no cheapening of these products.

It was found that the sugar content of eighty-seven samples of ginger ale varied between seven and thirteen per cent, with one-half the samples between eight and ten per cent. The sugar content of one hundred seventy samples of sarsaparilla, birch beer, and root beer varied between seven and fifteen per cent, with one-half the samples between nine and eleven per cent, somewhat higher than that of the ginger ale. This excess water in ginger ale can not be regarded legally as adulteration.

Excess water, however, can be used solely for adulterating purpose if an artificial sweetener, such as saccharine,

is used to offset the reduction of sweetness by dilution. Saccharine is an organic chemical of no food value. One pound of saccharine costs $1.50, or about $1.33 per pound more than sugar. The sweetening power of a pound is equivalent, however, to $27.50 worth of sugar. A solution of one part of saccharine in 550 parts of water can be substituted for equal weights of sugar. The sugar content of twenty-four samples of soft drinks containing saccharine was found to vary between four and nine per cent. Taking the average of these figures and comparing it with the average of the 617 samples found to be free from saccharine, the average adulteration by the addition of water was forty-three per cent. The samples collected during the past three years have been free from saccharine.

Scallops, Oysters, and Clams. Scallops are muscles which open and shut the shells of the bivalve so called. The balance of the animal is discarded. This form of shellfish does not require the washing so necessary with other shellfish, and consequently can be sold without the addition of water. Many years ago, scallop dealers would put three gallons of scallops into a five-gallon tub, add two gallons of water, put on the head and deliver to the shipper. When the shipment reached its destination, the tub contained five gallons of large dry scallops. This type of adulteration is rare today. Oysters and the soft shell clam require a wash after shucking, but if washed too long, the product is characterized as soaked. The oyster business is "big business" of an interstate nature and as such is subject to the scrutiny of the Federal officials, of the officials of the State of production, who naturally do not care for any criticism of a product of their own production, and also of the officials of the State where the oysters are to be consumed. This scrutiny should reduce adulteration to the irreducible minimum, yet the notices of judgment under the Federal law occasionally report the results of seizures of oysters adulterated by the addition of water.

The soft shell clam is a product of the coastal waters of Rhode Island, Massachusetts, New Hampshire, Maine, and the Maritime Provinces, but the business is small compared with the oyster business. There is a distinction between washing and soaking. A certain amount of water is absorbed during the washing process, but if the clams are allowed to remain in the wash water there is a greater absorption of water together with a loss of some of the water soluble food material which is removed by the water. The washing of shucked clams is essential and, therefore, adulteration cannot begin until after a suitable washing period. The washed clams then become a manufactured article different from the clams in the shell. It is, in many instances, a difficult matter to state the exact point at which washing ceases and soaking begins.

SUMMARY OF ANALYSES OF 63 SAMPLES OF SOAKED SOFT SHELLED CLAMS

No. of samples	*Average solids*	*Average water soluble solids*
	%	%
11	15.4	7.1
14	14.5	6.5
19	13.4	6.0
5	12.6	5.2
4	11.8	5.0

The preceding table shows the analyses of fifty-three samples of soaked clams. Each figure represents the results of the average of the samples with 15, 14, 13, 12 and 11 per cent solids respectively.

Elmer R. Tobey has published the analyses of 203 samples of shucked and washed clams. He states: "Clams

opened under proper conditions, washed but not soaked, show an analysis of free liquids not more than ten per cent, solids on the drained meats not less than eighteen per cent." His analyses show, on the whole, clams of fairly high quality. There were but fifteen samples with more than ten per cent free liquids, although seventy-nine had solids less than eighteen per cent. If both figures are used to prove soaking, only eleven samples were soaked. If the free liquor is entirely drained from soaked clams, the drained meat would nevertheless retain water absorbed during the soaking process.

Figure 3 from Tobey's data shows the relation between the average free liquor and the average solids on the drained meats. The samples with the highest free liquor also had a high solid content. Possibly the liquor was the natural liquor rather than that obtained by washing.

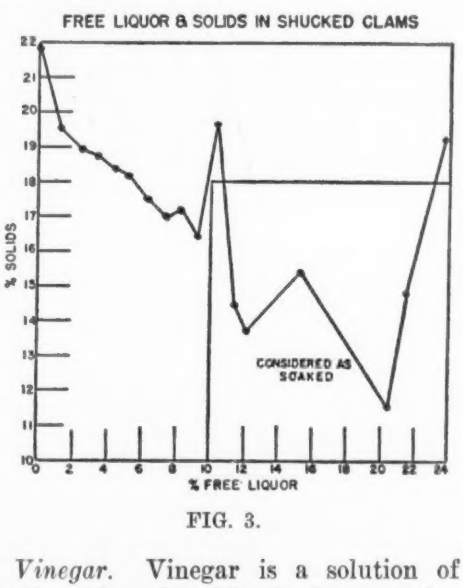


FIG. 3.

Vinegar. Vinegar is a solution of acetic acid produced by the fermentation of alcohol. The acetic acid content of vinegar depends upon the alcoholic content of the cider or wine from which it was made, together with the efficiency of the fermentation. The federal standard for vinegar is not less than four per cent acetic acid, and this standard is, either by law or regulation, the standard of practically all the states. Occasionally vinegar can be fermented to an acetic acid content of nearly six per cent. The standards and the law permit dilution to four per cent, provided the package is labeled "Diluted to Legal Strength," or words to that effect. The average vinegar sold at retail is thus diluted and is so labeled. If the standard were higher, say 4.5 per cent, which was formerly the standard of the State of Massachusetts, there would be a saving in freight which in the long run would be considerable, but would make a saving to the consumer of only a few cents per year.

A beverage called coffee. Owing to war conditions, transportation by water of food and other materials has become a somewhat hazardous occupation, resulting in a shortage of many articles of daily use, of which coffee is one. When the consumer prior to rationing was too often deprived of his daily cups of coffee in his home, he found that he could get "coffee" at the restaurants and acted accordingly. Because of this anomaly, an investigation was made which showed that many restaurants were not serving coffee, but a beverage made from a mixture of coffee, roasted chicory, peas, pea hulls, wheat, and so on. Investigation showed that the coffee dealers had sold the adulterated material, properly labeled and billed as such, but the restaurateur was willing, and even anxious, to sell the beverage as pure coffee and often urged his patrons to have some coffee when such had not been ordered.

The pure beverage coffee first purchased was from thirteen different restaurants, and it had a concentration of from 0.5 to 1.4 grams of solids per hundred cubic centimeters, averaging 1.02. The next twenty-six samples, made from pure coffee and collected a short

time later, varied in concentration from 0.5 to 1.2 grams of solids per hundred cubic centimeters, with an average of 0.8 grams per hundred cubic centimeters. This represents a dilution with water to an extent of nearly twenty per cent.

Flavoring extracts. Flavoring extracts, such as lemon, orange, peppermint, wintergreen and a few others, are made by dissolving the necessary quantity of the essential oil in ninety-five per cent alcohol. The greater cost is often in the alcohol, which varies with the internal revenue tax. These articles were often adulterated by the addition of three or four volumes of water. This precipitated the oil which was removed by filtration after the addition of an absorbent, such as magnesia. This product has been in part legalized under the name of terpeneless extract, and standards for the concentration of citral have been made for terpeneless lemon and terpeneless orange extracts. They are decidedly inferior to the undiluted extracts.

Alcoholic beverages. Alcoholic beverages are so constituted that watering can be practiced sometimes to the preference, as well as to the welfare, of the consumer.

Distilled liquor is popularly supposed to be 100 proof, that is, to contain fifty per cent alcohol, although the popular preference is for the blended whiskey of 90 proof, or forty-five per cent alcohol.

Between 1895 and 1901, the Massachusetts Department of Public Health examined eighty-two samples of whiskey obtained from drug stores, all of which were supposed to conform to the requirements of the United States Pharmacopoeia. Of these samples, 29.3 per cent contained less, and 8.5 per cent contained more, alcohol than the pharmacopoeia prescribed for whiskey.

In 1904, the Police Commissioner of Boston submitted sixty samples of whiskey. The analyses disclosed the fact that the worst ingredient from the health standpoint was alcohol, and that the most extensive adulterant was water. Seventy-three per cent of this whiskey contained added water, 66.3 per cent to an extent of 6.5 per cent or more, and 5 per cent to an extent of 29 per cent or more.

In 1923, the police departments of the State submitted 3,746 samples of distilled liquor. Here again it was found that the most injurious ingredient was alcohol, and the greatest adulterant was water. Of these samples, 67 per cent contained added water, 40 per cent to an extent of 9.5 per cent or more; 20 per cent to an extent of 25 per cent or more; and 7 per cent to an extent of 30 per cent or more.

Samples of whiskey recently purchased from saloons in Massachusetts were found, upon examination, to vary from 79.4 to 86.2 proof, averaging 82 proof. The United States Pharmacopoeia and the bottled in bond whiskey is required to be 100 proof, but the pre-prohibition blends were usually 90 proof. These recent samples, if based upon a standard of 100 proof, contained from 13.8 to 20.6 per cent added water, but if based upon a standard of 90 proof, contained from 2.7 to 11.8 per cent added water.

Drugs

Tincture of ginger. Tincture of ginger prepared as described in the United States Pharmacopoeia contains in each liter the alcohol soluble materials from 200 grams of ginger dissolved in ninety per cent alcohol. This substance has been used for years by many people solely because of its alcohol content. In order to furnish a material more suitable for beverage purposes, it was a common practice to manufacture a tincture of ginger of lower alcoholic content. This diluted material often was put up in very attractive bottles, some of which were labeled, "Picnic Flasks," and was frequently sold in Massachusetts towns

where the citizens had voted not to grant licenses for the sale of intoxicating liquor.

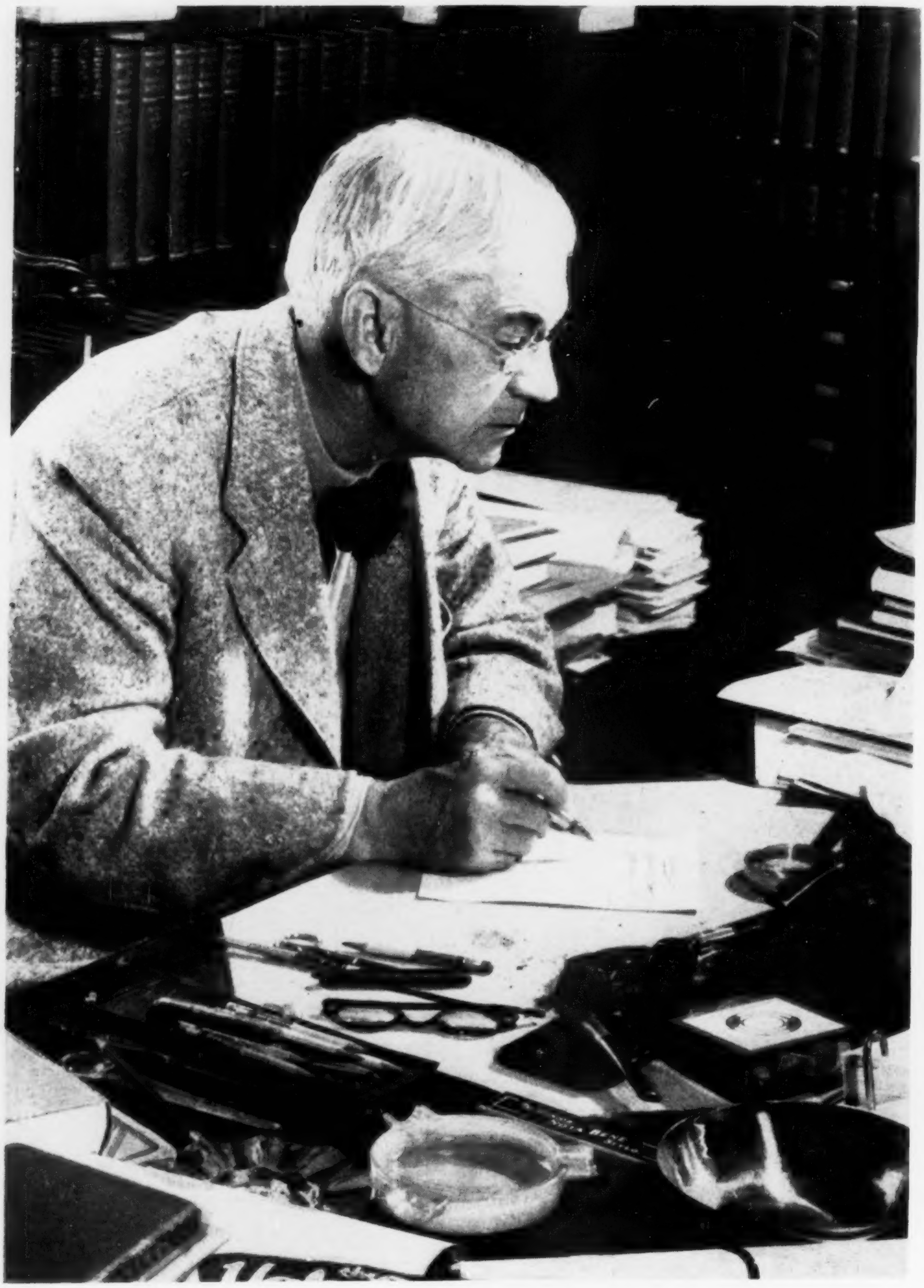
Double strength ginger extract containing the full complement of alcohol, even if more or less deficient in ginger resins, appeared during the prohibition era as the result of a Treasury Department's decision. The human animal soon demonstrated his ability to drink undiluted double strength tincture of ginger. The courts have recognized tincture of ginger as being an intoxicating liquor, within the meaning of the laws of Massachusetts.

Spirits and tinctures. Some of the foreign pharmacopoeias prescribe 80 per cent alcohol as well as 95 per cent alcohol in making certain spirits and tinctures. For example, the French Pharmacopoeia prescribes 90 per cent alcohol for making spirit of peppermint, while the United States Pharmacopoeia prescribes 95 per cent. Quite often the alcohol-soluble substance will dissolve as readily in 80 per cent alcohol as in 95 per cent alcohol, and the tincture or spirit so prepared will be as good in therapeutic activity as if made from the stronger alcohol. Spirit of camphor, up to the requirements in camphor content but made with 75 per cent instead of 95 per cent alcohol, was occasionally sold in Massachusetts twenty-five years ago. There was a saving in tax, a reduction in cost, but no reduction in the active drug.

New and Unusual Uses of Water as an Adulterant. Notices of judgment published by the United States Food Administration during the past few years have shown new methods of using water as an adulterant. For example, there have been several seizures of poultry into which water has been injected so as to increase its weight. There have been seizures of canned cat and dog foods. One such shipment was labeled to indicate that it contained significant amounts of meat, meat products, and carrots. Adulteration was alleged in that valuable constituents, that is, meat, meat products, and carrots, had been wholly or in part omitted from the article and water added. There have been seizures of sauerkraut juice containing, in one instance, only about half the minimum amount of lactic acid that properly made sauerkraut juice should contain, alleging that water had been substituted wholly or in part for sauerkraut juice.

Possible Adulteration by Water. Many complaints have been received of alleged adulteration of sweet cider by the addition of water. Although these complaints have been investigated, sufficient evidence has never been obtained to warrant further procedure. Manufacturers of broken-out eggs have complained that their competitors have been adding water to broken-out egg whites. Investigation showed normal composition in each instance. There are, however, a few untouched fields worthy of study and investigation, such as tomato juice and orange juice (not orangeade), served by hotels and restaurants.

Numerical Standards. The elimination of water as an adulterant is, to some extent, augmented by the establishment of numerical standards. Examples are: the fat standard for butter; the standard for total solids, or solids exclusive of fat, for milk; maple syrup to be concentrated to a weight of 11 pounds per gallon; bread not more than 38 per cent moisture; macaroni and similar articles not more than 13 per cent moisture; dried milk not more than 5 per cent moisture.



WILLIAM LYON PHELPS (1865–1943)

WILLIAM LYON PHELPS, 1865–1943

"How good is man's life, the mere living!
how fit to employ
All the heart and the soul and the senses
forever in joy."

Browning: *Saul.*

For half a century, William Lyon Phelps has been a familiar and beloved figure on the academic campus and on the American scene. He has found both fields alike open and challenging. To both he has brought a glad and vital interpretation of literature and life. His conception of the rights to individual life, liberty, and the pursuit of happiness has been fulfilled in terms not of Ivory Towers and Elysian Fields but of the active and abundant life.

Ever since he first began to teach English literature at Yale in 1892, and presently gave his first public lectures, his personal magnetism has attracted followers and friends beyond number. The unfolding story of his instant and constant success in classroom and on public platform has become proverbial. His direct and persuasive influence in enhancing knowledge and appreciation of literature and the fine arts generally has been widely felt and recognized. Universities and colleges throughout the country have delighted to honor him with distinctions and titles extraordinary in number and diversity. But no less manifest have been his clear and constant titles to popular esteem and to personal affection that far exceeded academic limits.

The range of his interests and the warmth of his sympathies brought him close to the minds and hearts of listeners of all types and tastes. Whether in the academic robe of his accustomed office as Public Orator at University Commencements, or in the informal light homespun so familiar to those who thronged his popular lectures, he was ever at home with his subject and with his audience. None knew better how to establish quick accord between speaker and hearer. Ready wit and natural friendliness of tone and manner counted in first impressions as certainly as infectious enthusiasm and unfeigned sincerity confirmed and strengthened them. With him the spoken word took on warmth and color and colloquial charm.

His conversational manner, in turn, encouraged the ready give-and-take of the question-and-answer period which he early added to his literary lectures. The printed prospectus of his first considerable series of public lectures in 1897 is significantly entitled *A Course of Reading in Nineteenth Century Poetry.* Each lecture is set forth in main outlines and with full accompanying list of reading. The final note is characteristic: "The members should do the reading before each lecture, making notes of anything they wish to have explained or discussed, and it is earnestly hoped that either during or after each lecture everyone will feel perfectly free to ask questions, make suggestions, or oppose the lecturer's opinions. Questions that require a fuller discussion may be submitted or mailed to Professor Phelps, and they will be answered for the benefit of the class at the beginning of the following lecture." A born teacher, he was free from formalism and dogmatism. To him teaching was not so much a profession as a passion. He ardently coveted for others his own insatiable thirst for reading and in rare measure he gave to others generous guidance and the sense of comradeship in novel and rewarding

adventures in the world of men and of books.

As within the college classroom Professor Phelps made the spoken word the quick medium of intimate accord with his students, so in a steady stream of books and periodical articles, he made the written word a flexible means of quickening the perceptions and sympathies of his readers. "My task which I am trying to achieve," wrote Joseph Conrad in defining his aim as creative artist, "is, by the power of the written word to make you hear, to make you feel—it is, before all, to make you *see*. That—and no more—and it is everything." Conrad's creed does not lose meaning, if applied to the like, though lesser, art of re-creative interpretation of creative literature. Of such revivifying power Professor Phelps was a lifelong exemplar, for he could unstop deaf ears and open blind eyes.

As readily as he moved his hearers to participate in classroom or public forum discussions, did he stir his readers to respond with written queries and comments. Month by month, for many years, his section of *Scribner's Magazine*, entitled "As I Like It," reflected far more than the personal tastes and opinions of its author. Under his guidance it became a clearing-house for the interchange of diverse views and for mutual enjoyment of the interplay of many minds. He himself cultivated and personified the hospitable and yet independent spirit.

In academic parlance, the special field of Professor Phelps was *Litterae Humaniores*. Of his many doctoral titles none fitted him so well as that of Doctor of Humane Letters. He had, indeed, some qualities in common with the scientist—eager intellectual curiosity, courage in daring experiments, and zest for new discoveries. He was a pioneer in various fields of study and teaching, as when he opened revolutionary courses in contemporary fiction and drama. But he was a modernist with deep reverence for the inheritance of the past. His final addresses, one to the members of a Yale Residential College, another to the Baccalaureate audience at New York University, were alike arresting interpretations of the undying values of the classics of literature as revealed in Homer and Shakespeare. From the outset to the end, William Lyon Phelps remained an interpreter who could reconcile warring impulses and elements through single-minded and wholehearted faith in the humanities of life and learning.

George H. Nettleton
Lampson Professor of English, Emeritus,
Yale University

THE PROGRESS OF SCIENCE

CONCENTRATIONS OF VITAMIN A, CAROTENE, AND XANTHOPHYLL IN NORMAL HUMAN BLOOD

EVERYONE realizes that the provision of suitable food for the armed forces and civilians at home is one of the most important factors in winning the war. Since World War I, we have learned that in addition to sufficient amounts of fats, proteins, carbohydrates (sugar, starch) and minerals, the diet must also include vitamins.

In our laboratory, we have been especially interested in studying vitamin A. This vitamin is essential for the growth and well-being of man and prevents the disease of the eyes known as xerophthalmia. This vitamin is found in such foods as butter, milk, eggs, liver and fish liver oils. In the first World War, many children in Denmark became permanently blind because the butter produced in Denmark was exported to Germany, and only the skim milk, from which the vitamin A had been removed, was left for the children to drink.

For some years investigators believed that vitamin A was present in the blood of man and animals, but this was difficult to prove because the amount in the blood is very small and because it is associated with large quantities of fat. Recently we have been able to establish the presence of vitamin A in human blood by adsorbing the vitamin on magnesium oxide and then determining its spectrophotometric absorption curve and the curve of its reaction product with the antimony trichloride reagent. This reagent gives a characteristic bright blue color with solutions of vitamin A.

While vitamin A is present normally in the blood of man and animals, the carotinoid pigments (carotene and xanthophyll) are found in the blood of only a few species: notably man, monkey, cattle and fowl. In man, vitamin A, carotene, and xanthophyll are found in the fluid part of the blood, or plasma, and not in the red cells. Vitamin A is an alcohol. Most of the vitamin A in the blood is in the free or alcohol form, but it is stored in the liver mainly esterified with fatty acids.

The yellow pigment carotene, or provitamin A, is changed in the body into vitamin A. It is found in carrots, yellow wax beans, orange juice, apricots, and many vegetables and fruits. Xanthophyll is another yellow pigment found in many vegetables, but we do not know of what use it is in human nutrition. Some of the beautiful colors of autumn leaves are due to carotene and xanthophyll.

Sometimes a person eats so many fruits and vegetables that his skin becomes quite yellow from these carotinoid pigments and the person is said to have "carotinemia." This is a harmless condition and the yellow color fades away when less of these foods is eaten.

Chemical methods have been developed for the determination of vitamin A, carotene, and xanthophyll in the blood and tissues of man and animals. In a recent study we have determined the concentrations of vitamin A, carotene, and xanthophyll in the blood of seven normal women and fourteen normal men the first of each month for the year August 1, 1942, to July 1, 1943. These subjects, who were eating a good average diet, were professors, interns, medical students and persons employed in the laboratories and offices of the University of Rochester Medical School. The concentrations of carotene and xanthophyll in the plasma were greatest during the autumn months, especially October, when fresh fruits and vegetables were plentiful and inexpensive. The

pigments were lowest in the late spring, in April and early May, when fresh fruits and vegetables were difficult to obtain. The pigments were probably lower than usual this year because the canned pigmented vegetables, such as peas, yellow corn and string beans, were rationed and people had to eat more of the colorless vegetables, such as white turnips, cabbage and onions, than they usually do. The vegetables being raised in hundreds of thousands of Victory Gardens will help greatly in supplying the carotene, or provitamin A, needed by everyone.

Surprisingly enough, the concentration of vitamin A in the blood of our subjects remained remarkably constant during the different seasons; that is, the vitamin A seemed to be independent of the amount of its provitamin, carotene, which was in the blood. We know, however, that in normal persons there is a large store of vitamin A in the liver. It may be that the healthy body is able to draw upon this store when vitamin A is lacking in the food, and so keep the vitamin A of the blood at the needed constant level throughout the year.

In the group of people studied, each person tended to keep his or her same relative position throughout the year; that is, a certain individual almost always had the highest levels of carotene, xanthophyll, and vitamin A, while another almost always had the lowest values. Some subjects had high vitamin A values and low pigment values, while other subjects had high pigments but low vitamin A. The differences in these values between our subjects seemed to be due to differences in the diets eaten, in the amount of pigments and vitamin A absorbed through the intestinal tract, and in the rate at which carotene was changed into vitamin A in the body.

The men had more vitamin A in their blood than the women. The average vitamin A for the year for one man was 224 International Units per 100 ml. of plasma, while the average for the group was 137 Units. This subject, in addition to eating an excellent diet, was accustomed to have a glass of sherry or some other alcoholic beverage with his dinner each night. We have shown that in man and dogs, vitamin A is mobilized from the liver into the blood by alcohol, and this fact may have been a partial explanation of this subject's high vitamin A.

One woman subject had very high carotinoid values, but the lowest average vitamin A—97 Units—of the group. We thought she was not utilizing or converting her carotene into vitamin A in a normal manner and that medication with thyroid might be indicated in her case.

Some of the subjects suffered occasionally from infections such as colds and the new disease, pneumonitis. During the acute stage of the illness, the vitamin A in the blood promptly fell to a low level, but after recovery soon returned to the normal value for that person. Indeed, fever of any kind, such as that associated with scarlet fever, pneumonia and tuberculosis, causes a rapid decrease of vitamin A and a slower decrease of carotene in the plasma. This suggests that during long continued illnesses, extra vitamin A should be given the patient.

However, in some diseases, such as those of the kidney, of the thyroid gland and diabetes, the vitamin A or pigments of the blood may be much greater than normal. In other diseases, such as those of the liver and intestinal tract, the patient may not be able to absorb vitamin A when given by mouth and it is desirable to inject the vitamin directly into the muscles.

A study of the vitamin A and carotinoid pigments of the blood is of aid in improving the nutrition of man and in diagnosing and treating his diseases.

S. W. Clausen and A. B. McCoord

HIGHWAYS OF STEEL

THE steel highway may be among the numerous technological inventions made for the battlefront that will become part of the fabric of everyday life in the post-war era.

Our armed forces have found the portable emergency landing mat of incalculable value, particularly in the jungle terrain of the Solomons, on the sand beaches of the Mediterranean and on the frozen land of the Aleutians. Out of these easily laid and hard wearing steel mats came the idea of the prefabricated steel highway which was recently projected by the Irving Subway Grating Company of Long Island City and by the township of Darien, Connecticut. As a joint experiment to prove the practicability of entire roads of steel grid, a steel roadway strip, 48 feet long and 22 feet wide, was constructed as a section in a highway where it will be put to test under normal passenger and truck traffic conditions. If it proves durable under such heavy wear it may become the prototype for a network of steel secondary roads throughout the Western Hemisphere.

The technique employed for the experimental installation involves attaching steel grating panels, each 2 feet by 12½ feet, filling the mesh with ordinary construction sand, and applying a coating of road oil. The mats are fastened together at the ends for stability; anchorage is effected by bent ends.

Steel grating of a similar type is now used as bridge decking and has proved to be remarkably non-skid in character. Steel surfacing of truck roadways connected with factory plants are also being

LAYING STEEL GRATING MATS FOR HIGHWAY TEST AT DARIEN, CONN.

FILLING INTERSTICES WITH SAND BEFORE COATING WITH ROAD OIL

used and these are said to have already qualified for durability.

The steel highway should be particularly useful in localities where the roads are frequently washed out or otherwise impaired by weather conditions or excessive wear and tear. Its model, the emergency landing mat for airplanes—because it is easily and quickly laid, portable, and affords excellent traction—is playing a crucial part in the war.

The steel mats may be envisioned as affording emergency roadways to be used in rushing aid to areas devastated by the war, by storm or flood, and in supplying paths over precipitous mountains, rain-soaked jungles or frozen tundra. The greatly increased steel capacity enforced by wartime needs will doubtless result in many still undreamed of uses of this vital material. Perhaps the new steel mats will provide one of the means necessary to maintain the difficult and chronically ailing passages of the Burma Road or to complete the Pan-American Highway, thus physically uniting the good neighbors of both Americas.

M. D.

THE NEW DIVISION OF ELECTRON AND ION OPTICS IN THE AMERICAN PHYSICAL SOCIETY

THE natural tendency of any growing science is to subdivide itself. The special topics developed at first by a few become the occupations of many workers, who find their respective specialties becoming so rich in knowledge and opportunity as to claim a steadily rising fraction of their energy and time. One of

the duties of a scientific society is to combat this tendency and to maintain the unity of its science as long as possible. This may seem a singular introduction to the statement that the American Physical Society, which for nearly fifty years has subsisted as a single body, has now commenced to organize a Division of Electron and Ion Optics.

Paradoxical as it may sound, this step is regarded by the officers and the Council of the Society as a step in the direction of unity and not in that of disintegration. The policy is not to deny the trend towards specialization but to keep the inevitable subdivisions within the framework of the Society so that those who specialize in them may continue to hold their meetings and their publications (to some extent at least) in common, and may continue to regard themselves and to be regarded as being physicists and members of a society of physicists. This policy is closely allied with that of increasing the part which industrial and applied physicists take in the meetings and other activities of the Physical Society: an important matter, now that the proportion of physicists engaged in industry and in the "borderline fields" is rapidly rising.

The opportunity for forming divisions within the American Physical Society was given several years ago by an amendment to the Constitution of the Society. Advantage has now been taken of it, owing in the main to the initiative of L. Marton of Stanford University, who undertook the requisite preliminary step of formulating a petition to the Council and winning the signatures of numerous members. The Division of Electron and Ion Optics is now in process of organization, and will probably make its debut as sponsor of a special program at one of the general meetings of the Society to be held during the coming winter and spring.

The following statement of the scope and object of the Division of Electron and Ion Optics has been prepared mainly by Dr. Marton.

"Electron and Ion Optics" is a relatively recent branch of physics. Some of its fundamental facts were known for a considerable time before its name was coined; but its proper history commences only with the last ten or fifteen years. During this recent period the optical analogies, existing between the behavior of a beam of light passing through refracting media and that of a beam of electrons passing through suitably shaped electric or magnetic fields, have been clearly recognized. The discovery that electrons and ions can be focussed by radially symmetrical fields, that images can be produced by such fields and that the action of any field can be described in terms of geometrical optics, was startlingly new. Such optical analogies helped in the better understanding of earlier observations, but much more than this, they started a very rapid development in a number of fields. Already electron and ion optics comprises theoretical, experimental and applied branches. The task of the theoretical physicists is to calculate the path of electrons or ions in electric or magnetic fields, to calculate new combinations with reduced aberration, to develop the theory of such effects which enter into the operation of electron or ion optical devices (such as, for instance, electron and ion scattering, diffraction, etc.). On the experimental side we can quote the investigation of electron or ion optical systems, the observation and measurement of the aberrations, and the design and practical realization of suitable fields. The work of the experimental electron optician toward newer and better systems very often overlaps with some problem of applied electron or ion optics.

The products of applied electron and ion optics are very manifold. A few of them have been widely publicized, such as the electron microscope, the cyclotron and other apparatus of transmutation,

many kinds of radio tubes and television apparatus. Other less widely publicized instruments are, however, very important in physical or other research; among these are mass spectrographs, cathode ray tubes and oscillographs, and electron diffraction apparatus. All such instruments have in common the characteristic that in them electrons or ions are accelerated, and the paths of these are modified by means of electric or magnetic fields in such a way that some kind of "focussing" of the beam is achieved, thus justifying the name "optics."

The field of the newly formed Division of Electron and Ion Optics of the American Physical Society is defined to comprise all theories and all apparatus involving the forming, directing, shaping and focussing of beams of electrons and ions. This definition is intended to make clear that the Division is not restricted to any special applications (such as, for instance, electron microscopy alone). Presumably the intention has been realized, as nearly five hundred members of the Society have already enrolled as members of the Division. Though the scope of the Division is thus carefully defined, it is not intended in the least to exclude any member of the Society who is interested enough to join; and nothing will preclude anyone who has joined this Division from joining any others which may later be formed. It should be stressed, also, that the function of the new Division is not a duplication of that of any already existing Society devoted to a special field of applied electron optics, but a grouping of those who are interested in the physics of electron-optical and ion-optical apparatus.

KARL K. DARROW

NATIONAL CONSUMERS FOOD CONFERENCE AT CLEVELAND

THE National Consumers Food Conference in Cleveland, Ohio, October 27 and 28, 1943, was a significant and interesting move on the part of some eighty large national organizations of consumers to inform themselves on international and domestic food distribution and production problems. The significant point is that the Conference was not planned, promoted or controlled by any government agency or commercial interest. Instead, it was a manifestation on the part of a wide variety of citizens' groups that it is time for the consumer to take a hand in setting the food policy which affects him so profoundly.

The Conference was sponsored by Food For Freedom, Inc., a voluntary non-partisan committee of private individuals, under the chairmanship of Mrs. Dwight W. Morrow, representing a cross section of citizens' organizations. It was initiated by a timid letter of inquiry which went to organizations representing national education groups and university women, to church, labor and racial organizations, to settlements, and to other civic and professional associations. This letter served as a preliminary feeler to see if the general public was interested in having a National Consumers Food Conference. For the most part, the response from the officers of these organizations was so enthusiastic that Food For Freedom decided to go ahead with plans for the Conference with the advice and cooperation of a "steering committee" composed of representatives of the most interested of these organizations.

The Conference was set up in a series of seven round tables to consider respectively, the following questions: How can we really hold food prices down? How well is our food distribution working? How is the war worker feeding himself and his family? How can we make the best use of our food in wartime and after the war? How can we step up home production and conservation of food? Can the consumer help to get

maximum production? What can we do to help supply the food needs of our allies and of liberated peoples?

The final recommendations from the Round Tables were given to all the delegates at a general session and were to be used as a basis for individual action by each national organization as it sees fit. In order that the discussions might attain the fullest public value, the Conference was so arranged that no Round Table chairman or delegates were federal or state officials or representatives of large commercial food trade interests. As Dr. William Allan Neilson, Chairman of the Board of Food For Freedom, said while presiding at the Mass Meeting at the Cleveland Public Auditorium:

Such an educational campaign as we have in mind cannot best be carried on by any government agency because of the understandable limitations controlling such agencies . . . nor can this type of educational activity squarely face the issue presented by the critical food situation abroad if it is attempted by a group under the domination of the food trade interests.

The importance of free discussion on the part of free citizens was recognized as one of the precious attributes of a free country which should be constantly exercised in the face of a wartime tendency to sit back and "let the government decide" without an expression on the part of the people. Government experts were invited to attend the conference only as consultants and were called upon for information but did not dominate or control the discussions. In this manner it was hoped that the reports would reflect the thinking of the representatives of the eighty national organizations present and that the recommendations made would be in the larger public interest.

Some passages in the reports are of special significance, such as the following by Miss Elizabeth Magee, Secretary of the National Consumers League:

Consumers can do much, which they are not now doing, to promote effective prosecution of the war and the laying of the foundation for a just and lasting peace by demanding maximum production and total use of all resources, both material and human. To give lasting meaning to the aims for which this war is being fought, they should demand that the adequate production and total use of all resources be carried over into the peace period. Within agriculture, this should result in abundant production and total use of farm resources. Because it offers the best hope of maximum production and the surest base for economic, social and political stability and health, consumers should support legislation and other efforts to make possible the existence of family type farming as the pattern of American agriculture.

One central theme permeated the seven Conference Round Tables and the speeches of Vice-President Wallace, Dr. Neilson, Mrs. Morrow, Dr. Theodore Schultz of the University of Chicago, Morris Rosenthal, former Deputy Director of the Board of Economic Warfare, Leon Henderson, former OPA Administrator and General Chairman of the Conference, President James G. Patton of the National Farmers Union, and Victor Reuther of the United Auto Workers. That theme was also the conclusion of the United Nations Food Conference at Hot Springs, Virginia, namely, that in the years immediately ahead there is no possibility of producing too much food anywhere in the world and that, conversely, the need is extreme for swiftly turning acreage into essential food production wherever that acreage holds promise of a yield worth the effort.

The Conference recommended at the final general session that Food For Freedom, Inc., continue its work by carrying on a nation-wide study and educational campaign through a series of regional and local conferences patterned after the Cleveland Conference. It was recognized that such an undertaking is vitally needed to familiarize the people of this country with the role increased production and improved distribution of food can and must be made to play in building an enduring and democratic peace.

Florence R. Wyckoff

BOOKS ON SCIENCE

INTRODUCTION TO CELESTIAL NAVIGATION*

THIS *Primer of Celestial Navigation* by Dr. Favill is the kind of book desired by one who wishes to know something of the whys and wherefores as well as the technique of determining a position at sea or in the air from observations of the sun and stars. As stated in the introduction, Dr. Favill fully appreciates that, with modern short-cut methods utilizing the Air Almanac and such convenient tables as Hydrographic Office Publications Number 214, anyone of ordinary intelligence can be quickly taught the procedures for observing altitude of the sun and stars and determining in a very few minutes therefrom his latitude and longitude. A student with an intelligent curiosity, however, may well have time enough at his disposal to ask many fundamental questions which this book will answer.

After an appropriate introduction, the author presents the fundamental astronomical concepts which underly the science and practice of navigation. Since it is apparent from the contents of the book that the author acquired his proficiency in navigation as a pastime, he has an appreciation for some of the difficulties encountered by the student who undertakes the subject by himself without formal training in mathematics or science, and in general, his exposition of fundamental concepts is all that such a student could desire.

To avoid confusion in the main text, the author has relegated numerical problems to the end of the book. A bibliography and a comprehensive index close the volume.

The book represents a fairly comprehensive introduction to celestial navigation and is a running commentary on the subject matter of the classic *American Practical Navigator* by Nathaniel Bowditch. It does not pretend to replace Bowditch nor does it include tables for the working of problems. These must be purchased separately from among those recommended. For the purpose of an accelerated course in navigation to meet the present war emergency, the book contains too much material and the presentation of so many methods would seem to be confusing rather than otherwise to the student whose time available for navigation is reduced to a minimum. The volume, however, should prove of interest to those desiring more knowledge of the art than is obtained from mastering short-cut methods in an emergency course. The book contains 263 pages and many diagrams. Its small dimensions commend it for accessibility and convenience. It is a useful addition to any nautical library.

HARLAN T. STETSON

* *Primer of Celestial Navigation.* John Favill. 46 ill. xvii + 263 pp. 2nd edition. 1943. $2.00. Cornell Maritime.

ELEMENTARY ELECTRONICS*

RAYMOND F. YATES is a prolific and deft writer of "popular science" in the quasi-engineering magazines that sell so well among non-college youth. As editor of *Modern Mechanics and Inventions* he knows their technical tastes and limitations. He has catered to their avid need of self-education by a dozen books on radio, model boats, microscopy, model engines and trains, and the "art" of inventing. "Super-electricity" is another such book. Its sub-title is "What You Can Do in Electronics." It is, however, no textbook. It expounds the first principles with a few elementary diagrams, lists hundreds of industrial and commercial uses of electronics; advises the

* *Super-Electricity.* Raymond F. Yates. Ill. 165 pp. $2.00. September, 1942. New York. D. Appleton-Century Company.

amateur on how to turn his hobby into a business and how to find a job; recommends practical books for home study; discusses electrical engineering training in colleges; and describes in some detail the excellent practical courses in electronics given by the R.C.A. Institutes. The high-school boy or girl who has a leaning toward radio can get a sketchy but realistic idea of what to do next from this book but will get no idea of the hard work and long study needed to achieve mastery of electronics.

GERALD WENDT

THE HISTORY OF CONTAGION*

THE Four Horsemen of the Apocalypse once more ride roughshod over humanity. War's boon companions—Famine, Pestilence and Death—stalk the world today. Their victims are legion and the end is not yet. In most past conflicts, Pestilence brought more victims into the arms of Death than War itself. Famine has already destroyed untold thousands. Many others will follow this winter. But Famine is earthbound and the dread agony of starvation spreads but very slowly compared to the winged speed of Pestilence. The menace of epidemic disease grows ever more acute as the war progresses. The crisis of threat will come *after* the last gun is fired. Then, once again, will occur free intercourse between peoples and the return homeward of many thousands of both conquerors and conquered. These hordes and legions of fight-weary civilians and soldiers will bring with them germs from far off places to populations unprotected by the immunization of long contact and often depleted by hunger, cold and fatigue. Fatigue will be much more apparent than now, when emotional drive spurs us on to hasten the day of victory and peace. But peace may bring devastating epidemics, unless enlightened social conscience and wise *scientific* administration effectively guard against infective diseases. Wishful thinking, emotional platitudes, catchy slogans and candied ideals will not be sufficient; only stark realism can initiate adequate defense activities. Even here in our "isolated" United States there is a serious probability that troops returning from the tropics and the cesspool of Europe will bring with them infected insect vectors capable of spreading typhus, plague, yellow fever or malaria with speed greater than that of a prairie fire before a strong wind. Infected hosts, even if free of insect vectors, may well infect native American carriers of contagion.

Such pandemics have been frequent and terrible aftermaths of war. The need for effective defenses is imperative. Intelligent planning, research and application of existing knowledge presuppose a thorough understanding of the past, for history has a way of repeating itself. A foreground without a background is an incomplete picture. A recent volume concerning the history of man's conquests over epidemic diseases, by Doctor Charles-Edward A. Winslow, Lauder Professor of Public Health at Yale University, is both timely and useful. Professor Winslow has succeeded in writing an interesting, authoritative and accurate volume which is liberally documented. The major theme is the history of man's ideas concerning the causation of epidemic disease. He traces the slow, painful evolution of etiologic thought from the earliest ideas of demonology, divine wrath, metaphysics, the early glimmerings of the concept of contagion and the first studies of epidemiology down through the epochal advances of Pasteur to modern knowledge. Three quarters of the four hundred pages are devoted to the eras preceding the advent of bacteriology.

More material dealing with the devel-

* *The Conquest of Epidemic Disease.* Charles-Edward A. Winslow. xii + 411 pp. 1943. $4.50. Princeton University Press.

opment of recent ideas of the etiology of epidemic diseases would be welcome to most readers. Professor Winslow has neglected to give his own significant contributions to epidemiology and sanitary science sufficient recognition. As a clinician, long concerned with individuals rather than with the "wholesale" attitude of Public Health, the reviewer regrets that there is not greater emphasis upon the important elements of individual resistance, relative immunity and individual health in the spread of epidemic diseases. This, however, may be a question of personal prejudice, for it is recognized that there are many who do not consider these important factors.

The Preface reveals that the author intentionally limited his discussion to the history of ideas of causation of epidemic disease, in the hope that the story of epidemiological thinking may throw light upon the pitfalls, blind bypaths and errors of scientific analysis in other fields. The advances of bacteriology have been magnificent and the benefits to mankind enormous. Nevertheless, in spite of the immense progress predicated upon the concepts of bacteriology, prolonged overemphasis of the rôle of the germ has, in some respects, retarded scientific development. The idea of specificity of the invading organism in the causation of specific diseases blinded many to the fact that disease occurs only when the germ finds haven in a vulnerable host. A seed without congenial soil does not produce. Causation is always a combination of multiple factors, which are clearly divisible into three categories: (1) predisposing, (2) provoking and (3) perpetuating influences. The infective agent is but one of these; study of the other factors has been badly handicapped by the asymmetric interest in bacterial and other parasites. Professor Winslow is one of the very few teachers of Public Health who openly recognize the limitations of the bacteriologic approach, particularly in considering future progress.

The book is highly recommended as a scholarly and most interesting volume. All who read it will profit thereby.

EDWARD J. STIEGLITZ

HANDBOOK OF PSYCHIATRY*

THIS volume, as its name suggests, aims to present the subject-matter of psychiatry in concise and simple terms. In this aim it succeeds in substantial measure. Intended primarily for medical students, the book is also prepared for all "whose work brings them in contact with mentally disturbed persons." Certainly the nurse and social worker can use it effectively, as can the college student of psychology.

The descriptions of the various forms of mental disorder are simply presented, without much discussion of the mechanisms involved. In general, the statements are accurate, except, perhaps, that the legal experience of the senior author crops out in his emphasis on the criminality of the psychopath and on the "drift toward crime" of the mental defective. With reference to the war psychoneuroses, too, one may question the need of an emotionally traumatic experience as the precipitating factor; too many cases are developing in our training camps to make that thesis hold!

The concluding chapters, particularly "Principles of Psychiatric Therapy," might be read with profit by everyone who has a mental patient in the family. A good index is provided, and a substantial bibliographical list follows each chapter.

The authors have presented information of value in condensed and readable form. The volume should have a good sale.

WINFRED OVERHOLSER

* *A Handbook of Psychiatry.* P. M. Lichtenstein and S. M. Small. 330 pp. 1943. $3.50. W. W. Norton.

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* Names of authors are in small capitals.

THE DECEMBER SCIENTIFIC MONTHLY

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PUBLISHED BY THE
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
SMITHSONIAN INSTITUTION BUILDING, WASHINGTON 25, D. C.

RECENT BOOKS OF SCIENTIFIC INTEREST

The War on Cancer. Edward Podolsky, M.D. 179 pp. $1.75. June, 1943. Reinhold.

Never before has the dramatic story of the relentless war being waged on this disease been presented so completely and intelligibly. It discusses the effectiveness of X-ray, Cyclotron, Radium, Surgery and Refrigeration and tells the very latest advances that science has made in the battle against this terrible killer—cancer.

Forward with Science. Rogers D. Rusk. Ill. xi + 307 pp. $3.50. April, 1943. Knopf.

Professor Rush's new book unfolds the amazing record of recent achievements in the field of physical science. Atom-smashing, artificial radioactivity, the electron microscope, such phenomena as cosmic rays, and the theories of relativity and quanta are analyzed in terms of their application to daily life.

The Amazing Petroleum Industry. V. A. Kalichevsky. Ill. 234 pp. $2.25. April, 1943. Reinhold.

To understand the importance of Petroleum in war and peace, read this fascinating book. It paints an interesting and vivid picture of the world's most important raw material—Petroleum. In nontechnical language it tells simply what petroleum is—how it is obtained—what it does.

Name That Animal. Ernest C. Driver. Ill. 527 pp. $5.00. 1942. E. C. Driver, Smith College, Northampton, Mass.

A guide to the identification of the common land and fresh-water animals of the United States, with keys, discussion of life histories, and selected bibliography for each major group except birds. Designed for courses in field zoology and for biology teachers interested in identifying animals.

Aerobiology. Edited by Forest Ray Moulton. Ill. x + 289 pp. $4.00. 1942. American Association.

Aerobiology is a specialized field which has been developed within the past 10 or 15 years. This volume is in two parts, extramural aerobiology and intramural aerobiology. Papers cover both the theoretical and practical aspects and references cover all important literature.

Chimpanzees. A Laboratory Colony. R. M. Yerkes. Ill. xi + 321 pp. $5.00. May, 1943, Yale.

This is a composite portrait of chimpanzee personality done in terms of behavior. It is of interest to the layman and the specialist in the field of psychological research. Many experiments took place at Orange Park, Fla., where Dr. Yerkes organized and for many years directed the Yale Laboratories of Primate Biology. June, 1943, selection of Scientific Book Club.

Mathematics Dictionary. G. James and R. C. James. Rev. Ed. Ill. viii + 319 pp. $3.00. 1943. Digest Press, Van Nuys, Calif.

Definitions of the basic words used in mathematics and an appendix of the tables needed in mathematics and its applications. The only such dictionary published. Both popular and technical definitions are given when feasible. An invaluable reference and time-saver for those who study or use mathematics.

Science Remakes Our World. James Stokley. Ill. $3.50. 1943. Ives Washburn, Inc.

A comprehensive survey of what is going on in the laboratories, and what the impact of new discoveries and processes will be on American daily living. Covers plastics, synthetics, radio, television, aeronautics, explosives, electronics, agriculture, vitamins, the sulfa drugs, atom-smashing, etc. The author is Technical Book Editor of the New York *Herald Tribune*.

Liebig and After Liebig. Edited by Forest Ray Moulton. viii + 111 pp. $3.00. 1942. A.A.A.S.

This is the title of a symposium presented at the 1940 meeting of the American Association for the Advancement of Science, commemorating the hundredth anniversary of the publication of Liebig's "Organic Chemistry in Applications to Agriculture and Physiology." Nine papers included in the volume.

The City—Its Growth—Its Decay—Its Future. Eliel Saarinen. Ill. by author. 379 pp. $3.50. May, 1943. Reinhold.

This unique work, the result of 40 years' experience points out the causes for urban decay, then sets forth a logical plan for future rehabilitation and decentralization. It conveys an important message to all concerned with progressive community planning and postwar development.

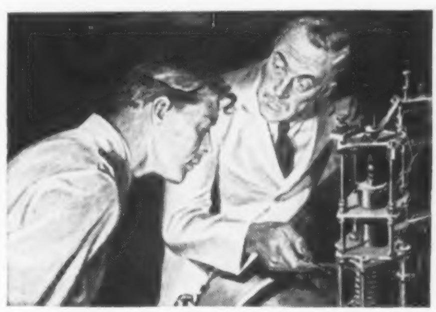
Relapsing Fever in the Americas. Edited by Forest Ray Moulton. Ill. vi + 130 pp. $3.00. 1942. American Association.

Includes 20 papers presented at a symposium on the subject. The papers constitute a comprehensive and documented discussion of relapsing fever as it occurs in the United States and Panama. Subjects covered include symptomatology, diagnosis, etiology, epidemiology, immunity, pathology, vectors, treatment and public health subjects.

Science and Criticism. The Humanistic Tradition in Contemporary Thought. H. J. Muller. xiv + 305 pp. $3.75. March, 1943. Yale.

"With a fine felicity of phrase and a brilliant clarity of insight, the Associate Professor of English in Purdue University looks at modern science from the point of view of a humanist. . . . Rarely does one find such a satisfactory synthesis of realism and idealism . . . recommended . . . to all . . . concerned with the trend of thought and action in this age of swift change in the life of man." *Scientific Book Club Review.*

Books previously announced will be given space six times on this page for $12.00



THE SCIENTIFIC MONTHLY

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An illustrated magazine broadly interpreting to the thoughtful public the progress of science and its relations to the problems confronting civilization. Published by the American Association for the Advancement of Science, Smithsonian Institution Building, Washington 25, D. C.

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Entered as second-class matter at the post office at Lancaster, Pa., U. S. A. July 18, 1923, under the Act of March 3, 1879.

NEW BOOKS OF SCIENTIFIC INTEREST

Race Riot. A. M. Lee and N. D. Humphrey. Ill. xi + 143 pp. 1943. $1.50. Dryden.

This book is based primarily on the Detroit race riots which were witnessed by both authors, sociologists at Wayne University, Detroit. In a vivid style they acutely analyze the conditions of race rioting and suggest specific measures for both basic and immediate corrective action in other danger spots.

Parasitic Diseases and American Participation in the War. Stunkard, Coggeshall, Makie, and Stoll. Pp. 189 + 262. 1943. New York Academy of Sciences.

The authors discuss the immediate and practical aspects of parasitology relating to the grave problems created by the war. They stress the need for education and stringent measures to curb the activity of pathogenic agents and disease vectors that may enter into this country through our troops returning from tropical and subtropical areas.

Finger Prints, Palms and Soles. Cummins and Midlo. Ill. xi + 309 pp. 1943. $4.00. Blakiston.

In this extensive study of dermatoglyphics, regarded as a relatively neglected aspect of human biology, patternings of epidermal ridges on fingers, palms, toes and soles are shown to have heritable traits and differential trends among races, constitutional types and between the sexes.

A Treasury of Science. Edited by Harlow Shapley, S. Rapport and H. Wright. xi + 716 pp. 1943. $3.95. Harper.

The general lay reader is invited by the editors of this anthology to participate in a series of exciting adventures in the development of science through the ages. The primary aim of the book is to assist "in the integration that seems essential to man's intelligent control of his own fabrications."

Learning to Care for Children. D. E. Bradbury and E. P. Amidon. Ill. ix + 149 pp. 1943. $1.25. Appleton-Century.

In the hope of decreasing the war manpower shortage without injury to the home, two child specialists have prepared this guide to high-school boys and girls who are encouraged to care for small children and thus release adults for work in war industries.

Principles and Practice of Rehabilitation. J. E. Davis. xxi + 211 pp. 1943. $3.00. Barnes.

Physical, mental, social and economic rehabilitation of the injured and handicapped is shown to depend largely on therapeutic cooperation. The author integrates the approaches of the work councellor and the psychiatrist and maintains that restoration in its most practical aspect is an adjustive process.

The Nature and Properties of Soils. T. L. Lyon and H. O. Buckman. 4/e. Ill. xi + 499 pp. 1943. $3.50. Macmillan.

The fourth edition of this college text of edaphology has been revised by Buckman. It contains additional material on the physics of soil moisture, chemistry of the colloidal state, and soil genesis and classification. The principles discussed in this valuable aid to crop production are applied to everyday problems.

Germans in the Conquest of America. Germán Arciniegas. Ill. 217 pp. 1943. $2.50. Macmillan.

Señor Arciniegas, well-known Colombian writer and diplomat, presents an engaging account of "a sixteenth century venture" involving German banking firms as well as the conquistadors from Spain and Portugal. He draws a parallel between the representatives of the German merchant-banks and present-day Nazi agents.